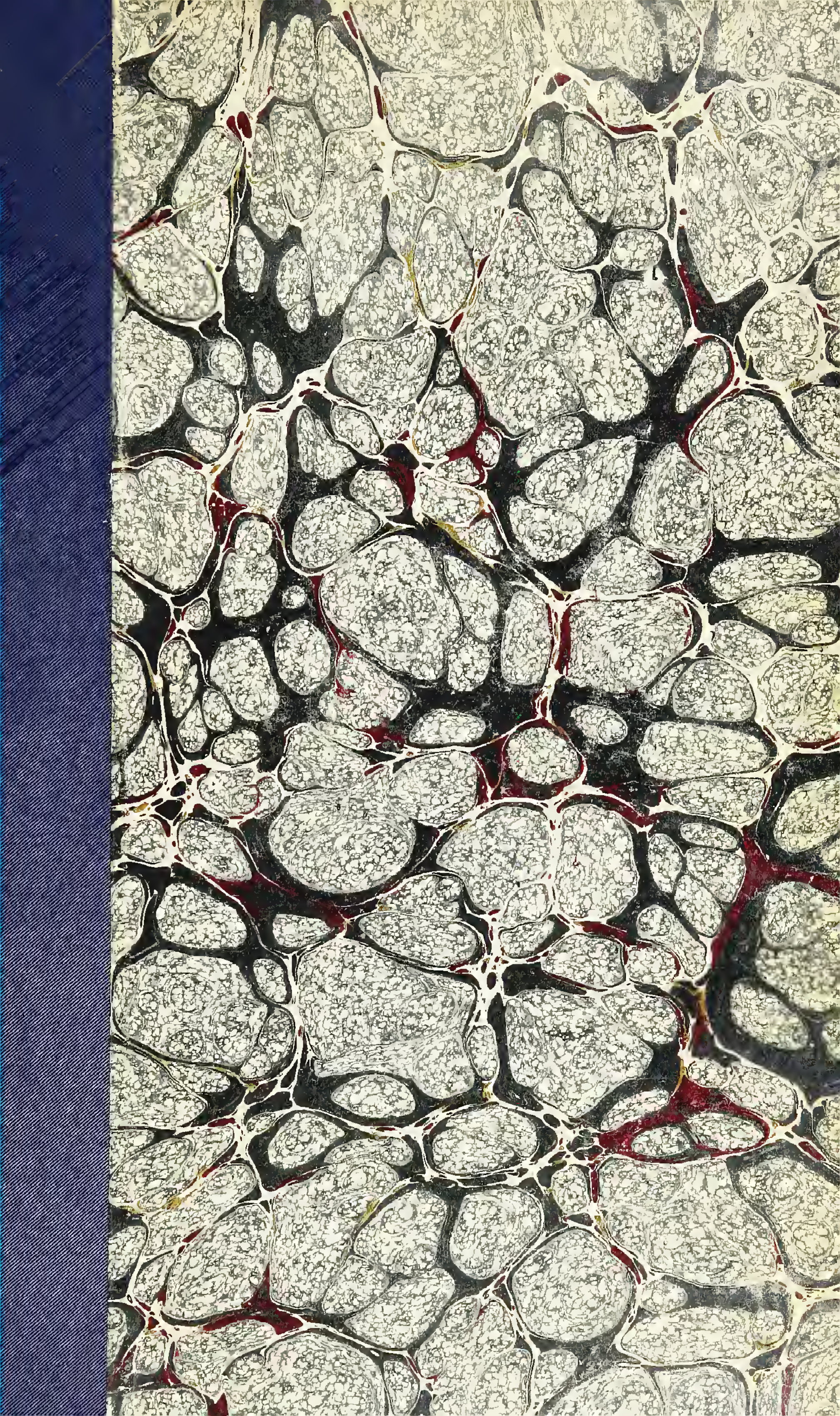

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MISCELLANEOUS SCIENTIFIC PAPERS OF THE ALLEGHENY
OBSERVATORY—NEW SERIES. No. 19

FRANK SCHLESINGER,
DIRECTOR.

Kindest-regards of the writer.

A BIOGRAPHICAL SKETCH OF S. P. LANGLEY

BY

JOHN A. BRASHEAR

REPRINTED FROM POPULAR ASTRONOMY, VOL. XIV, 1906
Northfield, Minnesota

S. P. Langley

SAMUEL PIERPONT LANGLEY.

JOHN A. BRASHEAR.

"The man of grand impulses sheds a lustre on all around him."

A great man has gone to his rest. A life filled with the highest aspirations has closed and we are left to mourn the loss of one whose place will be difficult to fill. The writer was associated with Professor Langley for more than thirty years, and in all those years he has only pleasant, aye delightful recollections of his personality, his magnificent intellect—one never satisfied with a half proven hypothesis—but always reaching out for final proof before he announced any of his great discoveries.

Not every man who came in contact with Professor Langley knew him as the writer knew him. Many times he has walked for miles with him. During the walk nothing would escape his lips but monosyllables. Was he cold—indifferent—callous to the questioner? Far from it. Some difficult—perhaps intricate problem in solar physics or other correlated study had taken possession of his mind to the exclusion of all else, and I have often thought that his "yes" or "no" to my questions were almost of an automatic character.

But how different at other times during our walks from the old Observatory to the nearest woods where now is erected the new Astronomical Observatory. Charming was his conversation from the beginning to the end of our stroll. When in this mood no man could be more entertaining and instructive than Professor Langley—indeed some of the most delightful remembrances of my long association with him come to me as I recall these delightful walks and talks.

When he was writing his New Astronomy—he would invite me to come to the Observatory in the evening and read to me a chapter of that splendid book. I call to mind the closing paragraphs of two that impressed me greatly, as he read them and for fear I may not quote verbatim I refer to his chapter on the Moon where, in closing his charming description of its scenery, he says: "Let us leave here the desolation about us, happy that we can come back at will to that world, our own familiar dwelling where the meadows are still green and the birds still sing, and where better yet still dwells our own kind — surely *the* world, of all we have found in our wanderings, which we should ourselves have chosen to be our home."

Let me also quote the closing paragraph of his chapter on the Stars, which I heard him read "in the long ago," a beautiful illustration of the life history of man as compared to that of the stars:—"I have read somewhere a story about a race of ephemeral insects who live about an hour. To those who are born in the early morning the sunrise is the time of youth. They die of old age while his beams are yet gathering force, and only their descendants live on to midday; while it is another race which sees the Sun decline, from that which saw him rise. Imagine the Sun about to set, and the whole nation of mites gathered under the shadow of some mushroom (to them ancient as the Sun itself) to hear what their wisest philosopher has to say of the gloomy prospect. If I remember aright, he first told them that, incredible as it might seem, there was not only a time in the world's youth when the mushroom itself was young, but that the Sun in those early ages was in the eastern, not in the western sky. Since then, he explained, the eyes of scientific ephemera had followed it, and established by induction from vast experience the great "Law of Nature," that it moved only westward; and he showed that since it was now nearing the western horizon, science itself pointed to the conclusion that it was about to disappear forever, together with the great race of ephemera for whom it was created.

" What his hearers thought of this discourse I do not remember, but I have heard that the Sun rose again the next morning."

Professor Langley was a lover of children. I have heard it from many friends whose homes he visited, how he would gather the little ones around him and tell them fairy stories, many of which he would improvise with wonderful tact to please the children.

A Washington lady had made several attempts to converse with Professor Langley—at receptions—upon his scientific investigations but failing to get the response desired, in her despair she asked him one evening what he did like to talk about. He quickly replied, "Children and fairy stories."

His beloved niece—the daughter of Professor John W. Langley who attended him in his last illness—told the writer that he took with him from Washington one of Andrew Lang's fairy story books which he read during his all too brief stay in South Carolina.

The writer of this brief sketch of Professor Langley's life feels totally inadequate for the task, but deems it a great pleasure to place on record some of the lovely traits of character on the side of "the humanities"—separate and distinct from his scientific work, for with all that apparent "calmest coldness" there was something reaching almost to the transcendental in his inner life—which my long acquaintance and association brought to light. I have given but a few instances of these pleasant reminiscences, instances which could be multiplied many times.

The writer can never forget the generous help given him in the solution of optical problems, from the very beginning of his acquaintance with Professor Langley. I never visited the Smithsonian Institution while he was there, that he did not call to my mind the first night we met, when, with fear and trembling, I unwrapped from a red bandana handkerchief my first five-inch objective on which my wife and I had worked the spare time of three years. We had met with many discouragements and failures but that night I received such generous words of encouragement that the subsequent work on the objective was made easier by far.

Several years afterward when the writer had the good fortune to discover a method of polishing the rocksalt lenses and prisms for his spectro-bolometric research—and giving them an accurate optical surface, his joy seemed to have no bounds, and I have letters from him expressive of his gratification, that are prized beyond measure.

It would be presumption for the writer to undertake to dilate upon the scientific labors of Professor Langley. They are too well known to the scientific world and especially to the readers of *Popular Astronomy*, to need repetition here, but a brief recapitulation of some of his more important work may not be without interest.

Professor Langley came by invitation to the Allegheny Observatory in 1867. Two years afterwards, 1869, he introduced standard time distribution to the cities and railroads. Inasmuch as there has been a great deal of controversy over the question of priority in this matter, a discussion of which would be out of place here, I will simply quote from an article written by Professor Langley more than twenty years ago.

"A mention of the Observatory's work would be incomplete without some account of its system of time distribution introduced by its present director in 1869. Previous to that date, time had been sent in occasional instances from American Observatories for public use but in a temporary or casual manner."

"The Allegheny system, inaugurated in that year, is believed to be the parent of the present ones used in this country in that it was so far as is known the first regular and systematic system of time distribution to railroads and cities adopting it as an official standard."

At the time this article was written signals were being sent over 4713 miles of railroad from the Allegheny Observatory. The closing paragraph of the article referred to will be of interest.

"For the benefit of any future writer of the history of the subject it may be stated that in 1870 the Allegheny Observatory had already in extended operation the system of time distribution above described; that about 1873 the director at Cambridge Observatory, after conference with the writer, introduced substantially the same provisions for connecting Harvard College Observatory with the New England roads; and that about the same time the Washington Observatory, which had previously sent signals in a limited and desultory manner, commenced to do so in emulation of the new system."

Professor Langley made good use of the income derived from the time service for the commencement of that long series of solar researches—which have added so much to our knowledge of the Sun. He was an exquisite draughtsman—as can be testified by the hundreds of beautiful drawings of solar phenomena made by him. Every student of Astronomy has seen reproductions of his charming drawing of the great sun-spot of December 1873—which has indeed become classic as a "typical" sun-spot. The sky of Allegheny with its murkiness often contributes to fine definition on the Sun, as the writer can testify, he having seen the delicate detail of the penumbral fringes come out like unto a steel engraving. The halcyon days of astronomical photography had not yet come when Professor Langley made his beautiful drawings.

Fortunately for Professor Langley's great work of the future, William Thaw, the staunch friend of the Observatory and one of the pioneers in its construction and equipment, became deeply interested in the director's solar work, and aided him in many ways. The income of the Observatory was so limited that little could be done in experimental research until William Thaw with a most liberal hand and heart provided the means for carrying on this great work, and your readers will find at the close of almost every monograph written by Professor Langley—"This research was made possible through the liberality of a citizen of Pittsburg." By Mr. Thaw's request his name was never mentioned.

In his studies in the domain of solar physics, Professor Langley was early impressed with the idea that much of the radiant energy from the Sun was not recognized by the instruments then in use and after a long series of experiments, discovered and developed that marvelously delicate instrument, the bolometer. With the bolometer a series of investigations were commenced upon the Sun, Moon and stars, which were continued for many years, bringing to light some of the most important facts in the whole realm of astronomical physics.

Professor Langley, with his assistants, Professor Keeler, Professor Very, and Mr. Page, continued the study of the hitherto unknown region of solar radiation until the bolographic chart, reaching far down into the spectrum, and showing almost innumerable curves of selective absorption, was given to the world, as a most valuable contribution to our knowledge of radiant energy. Although this great work was thought to be nearly completed before Professor Langley accepted the secretaryship of the Smithsonian Institution at Washington, he had continued the experimental research in the same field, at the astrophysical observatory connected with the Institution with a more refined equipment, but instead of the visual, using the photographic method, yet nothing has been developed to change the results of the Allegheny Observatory research. After years of study at Allegheny upon the problem of the selective absorption of the Earth's atmosphere at the lower levels, Professor Langley desired to make a similar investigation at a very high altitude. The top of Mt. Whitney in southern California was selected, as well as a station about two miles below the peak. William Thaw provided most of the means for the now famous expedition, our own government also sharing in the expenses of the research. Professor Keeler and Dr. William Day assisted Professor Langley in the work of this expedition, the results of which have now become classic, indeed have to a large degree settled the problem of the selective absorption of the Earth's atmosphere in its relations to the Sun's radiant energy, and the intimately correlated problem of life upon our globe. Professor Langley found time to study many minor though important questions bearing upon radiant energy, not alone from the Sun, but from other sources. His studies of the Moon's temperature added immensely to our knowledge of the "lesser light that rules the night". With the spectrobolometer, the highest temperature of the Moon was found to be about zero centigrade, and the lowest temperature not far from the temperature of space.

Professor Langley's work in mapping the invisible spectrum was a herculean task. Aided by Professors Keeler, Very and Page, thousands of measurements were made with the spectrobolometer and read visually, before the photographic method was devised and developed so beautifully by Professor Langley and his co-workers at the Smithsonian. Well do I remember how many times the rockslat trains were brought over to our workshop from the old Allegheny Observatory say about ten o'clock in the morning of a bright day, with the request that they be *polished and figured by noon* ! I do not remember ever to have disappointed these good people in the matter of their request.

The subsequent mapping of the infra red spectrum by photography at the Smithsonian Astrophysical Observatory has been a magnificent piece of work and to Professor Langley's assistant in later years, Mr. Abbot, must be given a share of the credit which he so well deserves.

More recently Professor Langley became deeply interested in the problem of *variable* solar radiation, in which he was greatly assisted by Mr. Abbot and his associates, and although the research had not been completed at the time of his death--he was convinced by results already obtained, that our Sun is a variable with rather a larger coefficient than has even been suspected.

Of his work in the domain of aerodromics the writer wishes to say a few words. The story is a long one of how Professor Langley became interested in the problem of flight—but suffice it to say here that his original purpose was *not* to construct a flying machine but if possible to determine the *laws governing flight*, and I am sure that Professor Very and others associated with Professor Langley in this most fascinating problem will bear me out in this that he undertook to solve it in a rigorously scientific manner. His invention of the dynamometer chronograph aided largely in this research. Nothing was left to guess at—every experiment with birds and aeroplanes was as carefully carried out as were his astrophysical studies. For three years the writer was associated with his friend Professor Langley in his investigations on this great problem, and I must add my testimony to that of others that everything was conducted in the true scientific spirit—and though in his later work on mechanical flight at the Smithsonian, success or failure cheered or depressed him—he has left on record most valuable facts that must be of inestimable value to those who desire to push forward this fascinating study.

Professor Langley received many honors during his life time. To be called as the successor of Professor Baird as secretary of the Smithsonian Institution was the highest gift in a scientific sense that could be conferred upon him by his country. Oxford conferred upon him the degree of D. C. L., Cambridge, the degree of D. Sc., Harvard, Princeton, University of Michigan and the University of Wisconsin, conferred upon him the degree of LL.D.

He was a correspondent of the French Academy, a Foreign member of the Royal Society, a Fellow of the Royal Astronomical Society, and other scientific associations. He received the Henry Draper, Count Rumford, Janssen, and other medals for his splendid discoveries.

Many of Professor Langley's works had great literary beauty. One has only to read the pages of his "New Astronomy" to be charmed with the beauty and clearness of his methods, and no matter how abstruse the subject, he seemed to have the ability to make it plain and thus appreciated even to the lay reader, who, though not an investigator, had a love for the subject discussed.

I quote from the words of a friend:

"Professor Langley had a deep interest in all that related to things of the mind, and was for many years associated with both the American and British Associations for Physical Research. He was an omnivorous reader of everything that was best in English and French literature, and was especially interested in George Borrow. He was the owner of a considerable collection of Borrow's manuscripts, and was a student of French history and memoirs. His interest in the fine arts was keen, and he had many times visited the galleries of Europe and knew the great pictures everywhere. The orient had a fascination for him, and he had a collection of the editions of the "Arabian Nights." As a young man he was a great admirer of Thomas Carlyle, with whom he had an acquaintance, and to whose home he made several pilgrimages. He had a great love for little children, and the "children's room" in the Smithsonian building was the result of his personal care and attention.

Professor Langley was born in Roxbury, Mass., August twenty-second, 1834. On the twenty-second of November last he suffered a slight stroke of paralysis but recovered sufficiently to be taken to Aiken, S. C., where he was attended by a faithful nurse and his beloved niece, the daughter of his brother, Professor John W.

Langley. Professor Langley wrote several letters from Aiken to his friends, particularly to those associated with him in the Smithsonian Institution, and great hopes were entertained that his recovery was only a matter of time, but it was not so to be. A second stroke came as "a thief in the night" but brought no pain to our friend—and after two days of apparently quiet sleep, broken at intervals—he passed away to the "Summer Land of Song," on the twenty-seventh day of February, 1906. His body was brought to Washington and the funeral services held in All Souls Church. The venerable Edward Everett Hale, an old time friend of Professor Langley, spoke most fitting and kindly words at the brief services. The Regents of the Smithsonian Institution acted as honorary pall bearers, and the members of the Smithsonian staff as active pall bearers.

The remains were taken to Boston in a private car, where they were laid away in Forest Hills Cemetery by the side of his mother. His intimate friend Dr. Adler informed the writer that this was Professor Langley's expressed wish. A short address was delivered at the cemetery by his friend Alexander Graham Bell, in which he gave a brief resumé of Professor Langley's life work.

When Napoleon stood at the edge of the battlefield where a great victory had been won by his soldiers, his ears were deaf to the groans of the wounded and dying, he heard them not, but said to his generals— in the ecstacy of victory

"C'est magnifique."

When our own Langley stood on the summit of the old Allegheny hills where so much of his life work had been done—and watched the Sun setting in all his glory, we heard him exclaim—"It is magnificent." Now that he is gone, and we look over the fields of science where he has won his grand but bloodless victories—we can but add our final tribute by repeating the words—

"It is magnificent."

Allegheny, Penn.

THE

HISTORY OF A DOCTRINE.

By S. P. LANGLEY.

[FROM THE AMERICAN JOURNAL OF SCIENCE, VOL. XXXVII, JAN., 1889.]

ART. I.—*The History of a Doctrine;* Address by S. P. LANGLEY, retiring President of the American Association for the Advancement of Science.*

> "Man, being the servant and interpreter of nature, can do and understand so much, and so much only, as he has observed, in fact or in thought, of the course of nature. Beyond this he neither knows anything nor can do anything."—BACON'S *Novum Organum*, aphorism 1.

IN these days, when a man can take but a very little portion of knowledge to be his province, it has become customary that your president's address shall deal with some limited topic, with which his own labors have made him familiar; and accordingly I have selected as my theme, the history of our present views about radiant energy, not only because of the intrinsic importance of the subject, but because the study of this energy in the form of radiant heat is one to which I have given special attention.

Just as the observing youth, who leaves his own household to look abroad for himself, comes back with the report that the world, after all, is very like his own family, so may the specialist, when he looks out from his own department, be surprised to find that, after all, the history of the narrowest specialty is strangely like that of scientific doctrine in general, and contains the same lessons for us. To find some of the most useful ones, it is important, however, to look with our own eyes at the very words of the masters themselves, and to take down the dusty copy of Newton, or Boyle, or Leslie, instead of a modern abstract; for, strange as it may seem, there is something of great moment in the original that has never yet been incorporated into any encyclopædia, something really essential in the words of the man himself which has not been indexed in any text-book, and never will be.

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* This Address appears here complete with the notes that have not hitherto been published.—EDS.

It is not for us, then, here to-day, to try

> "How index-learning turns no student pale,
> Yet holds the eel of science by the tail;"

but, on the contrary, to remark that from this index-learning, from these histories of science and summaries of its progress, we are apt to get wrong ideas of the very conditions on which this progress depends. We often hear it, for instance, likened to the march of an army towards some definite end; but this, it has seemed to me, is not the way science usually does move, but only the way it seems to move in the retrospective view of the compiler, who probably knows almost nothing of the real confusion, diversity, and retrograde motion of the individuals comprising the body, and only shows us such parts of it as he, looking backward from his present standpoint, now sees to have been in the right direction.

I believe this comparison of the progress of science to that of an army, which obeys an impulse from one head, has more error than truth in it; and, though all similes are more or less misleading, I would prefer to ask you to think rather of a moving crowd, where the direction of the whole comes somehow from the independent impulses of its individual members; not wholly unlike a pack of hounds, which, in the long-run, perhaps catches its game, but where, nevertheless, when at fault, each individual goes his own way, by scent not by sight, some running back and some forward; where the louder-voiced bring many to follow them, nearly as often in a wrong path as in a right one; where the entire pack even has been known to move off bodily on a false scent; for this, if a less dignified illustration, would be one which had the merit of having a truth in it, left out of sight by the writers of text-books.

At any rate, the actual movement has been tortuous, or often even retrograde, to a degree of which you will get no idea from the account in the text-book or encyclopædia, where, in the main, only the resultant of all these vacillating motions is given. With rare exceptions, the backward steps—that is, the errors and mistakes, which count in reality for nearly half, and sometimes for more than half, the whole—are left out of scientific history; and the reader, while he knows that mistakes have been made, has no just idea how intimately error and truth are mingled in a sort of chemical union, even in the work of the great discoverers, and how it is the test of time chiefly, which enables us to say which *is* progress, when the man himself could not. If this be a truism, it is one which is often forgotten, and which we shall do well to here keep before us.

This is not the occasion to review the vague speculations of the ancient natural philosophers from Aristotle to Zeno, or to give the opinion of the schoolmen on our subject. We take it up with the immediate predecessors of Newton, among whom we may have been prepared to expect some obscure recognition of heat as a mode of motion, but where it has been, to me at least, surprising, on consulting their original works, to find how general and how clear an anticipation of our modern doctrine may be fairly said to exist. Whether this early recognition be a legacy from the Lucretian philosophy, it is not necessary to here consider. The interesting fact, however it came about, is the extent to which seventeenth century thought is found to be occupied with views which we are apt to think very recent.

Descartes, in 1664, commences his "Le Monde" by a treatise on the propagation of light, and what we should now call radiant heat by vibrations, and further associates this view of heat as motion with the distinct additional conception, that in the cause of light and radiant heat we may expect to find something quite different from the sense of vision or of warmth ;* and he expresses himself with the aid of the same simile of sound employed by Draper over two hundred years later. The writings of Boyle on the mechanical production of heat †

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* "Me proposant de traiter ici de la lumière, la première chose dont je veux vous avertir est qu'il peut y avoir de la différence entre le sentiment que nous en avons, c'est-à-dire l'idée qui s'en forme en notre imagination par l'entremise de nos yeux, et ce qui est dans les objets qui produit en nous ce sentiment, c'est-à-dire ce qui est dans la flamme ou dans le soleil qui s'appelle du nom de lumiere: la plupart des philosophes assurent que le son n'est autre chose qu'un certain tremblement d'air qui vient frapper nos oreilles; en sorte que si le sens de l'ouïe rapportoit à notre pensée la vraie image de son objet, il faudroit, au lieu de nous faire concevoir le son, qu'il nous fît concevoir le mouvement des parties de l'air qui tremble pour lors contre nos oreilles. * * On passe doucement une plume sur les lèvres d'un enfant qui s'endort, et il sent qu'on le chatouille: pensez-vous que l'idée du chatouillement qu'il conçoit ressemble à quelque chose de ce qui est en cette plume?

† Detached passages from Bacon, Hobbes, and Locke, referring to the conception of heat, as a mode of motion, have been so often cited, that I will not repeat them here; but as these after all convey rather an impression of the acuteness of their authors, as men before their time, than the idea of a doctrine fully and clearly apprehended at the time, I prefer to offer the following much less known quotation, which I find in the works of Boyle, published *circa* 1670.

I beg the attention of the reader to the remarkable passages which follow, and which must have had, in Newton's day, all the currency which the eminent reputation of the author (a founder of the Royal Society) could give.

Extracts from the treatise on the "Mechanical Origin of Heat," by the Honorable Robert Boyle.—"Heat will appear the more likely to be mechanically producible, contain illustrations (like that of the hammer driving the nail, which grows hot in proportion as its bodily motion is arrested) which show a singularly complete apprehension of views we

if we consider the nature of it, which seems to consist mainly, if not only, in that mechanical affection of matter we call local motion mechanically modified, which modification, as far as I have observed, is made up of three conditions."

"The first of these is, that the agitation of the parts be vehement. Thus, in a heated iron, the vehement agitation of the parts may be easily inferred from the motion and hissing noise it imparts to drops of water, or spittle, that fall upon it. For it makes them hiss and boil, and quickly forces their particles to quit the form of a liquor, and fly into the air in the form of steams."

"The second is this, that the determinations be very various, some particles moving towards the right, some to the left hand, some directly upwards, some downwards, and some obliquely, etc. As a thoroughly ignited coal will appear every way red, and will melt wax, and kindle brimstone, whether the body be applied to the upper or to the lower, or to any other part of the burning coal. And congruously to this notion, though air and water be moved never so vehemently as in high winds and cataracts; yet we are not to expect that they should be manifestly hot, because the vehemency belongs to the progressive motion of the whole body."

"There is yet a third condition; namely, that the agitated particles, or at least the greatest number of them, be so minute, as to be singly insensible. For though a heap of sand, or dust itself, were vehemently and confusedly agitated by a whirlwind, the bulk of the grains or corpuscles would keep their agitation from being properly heat. If some attention be employed in considering the formerly proposed notion of the nature of heat, it may not be difficult to discern that the mechanical production of it may be divers ways affected. For by whatever ways the insensible parts of a body are put into a very confused and vehement agitation, by the same ways heat may be introduced into that body; agreeably to which doctrine, as there are several agents and operations by which this calorific motion (if I may so call it) may be excited, so there may be several ways of mechanically producing heat."

Boyle goes on to cite numerous experiments.

"Experiment VI. When, for example, a smith does hastily hammer a nail or such like piece of iron, the hammered metal will grow exceeding hot and yet there appears not anything to make it so, save the forcible motion of the hammer, which impresses a vehement and variously determined agitation of the small parts of the iron; which being a cold body before, by that superinduced commotion of its small parts becomes in divers senses hot. Again, if a somewhat large nail be driven by a hammer into a plank, or piece of wood, it will receive divers strokes on the head before it grow hot; but when it is driven to the head, so that it can go no further, a few strokes will suffice to give it a considerable heat; for whilst, at every blow of the hammer, the nail enters further and further into the wood, the motion that is produced is chiefly progressive, and is of the whole nail tending one way; whereas, when that motion is stopped, then the impulse given by the stroke, being unable either to drive the nail further on, or to destroy its entireness, must be spent in making a various vehement and intestine commotion of the parts among themselves, and in such an one we formerly observed the nature of heat to consist."

"Experiment VII. That I might also show that not only a sensible, but an intense degree of heat, may be produced in a piece of cold iron by local motion, I caused a bar of that metal to be nimbly hammered by two or three lusty men and these soon brought it to that degree of heat, that not only was it a great deal too hot to be safely touched, but probably would have kindled gun-powder."

Boyle goes on in the eighth experiment to illustrate the production of heat by friction by the use of a file, the whetting of a blade of a knife, the head of a piece of brass rubbed on the floor until it burns one's fingers, the heat of the axletree of a carriage by the friction of the wheel, and the common experiment of striking fire with a steel and flint, the latter as illustrating the instantaneity of the production of the heat.

are apt to think we have made our own; and it seems to me that any one who consults the originals will admit, that, though its full consequences have not been wrought out till our own time, yet the fundamental idea of heat as a mode of motion is so far from being a modern one, that it was announced in varying forms by Newton's immediate predecessors, by Descartes, by Bacon, by Hobbes, and in particular by Boyle, while Hooke and Huyghens merely continue their work, as at first does Newton himself.

If, however, Newton found the doctrine of vibrations already, so to speak, "in the air," we must, while recognizing that in the history of thought the new always has its root in the old, and that it is not given even to a Newton to create an absolutely new light, still admit that the full dawn of our subject properly begins with him, and admit, too, that it is a bright one, when we read in the "Optics" such passages as these:

"Do not all fixed bodies, when heated beyond a certain degree, emit light and shine, and is not this emission performed by the vibrating motions of their parts?" And again: "Do not several sorts of rays make vibrations of several bignesses?" And still again: "Is not the heat conveyed by the vibrations of a much subtler medium than air?"

Here is the undulatory theory; here is the connection of the ethereal vibrations with those of the material solid; here is "heat as a mode of motion;" here is the identity of radiant heat and light; here is the idea of wave-lengths. What a step forward this first one is! And the second?

The second is, as we know, backward. The second is the rejection of this, and the adoption of the corpuscular hypothesis, with which alone the name of Newton (a father of the undulatory theory) is, in the minds of most, associated to day.

Do not let us forget, however, that it was on the balancing of arguments from the facts then known, that he decided, and that perhaps it was rather an evidence of his superiority to Huyghens, that apprehending equally clearly with the latter the undulatory theory, he recognized also more clearly that this theory, as then understood, utterly failed to account for several of the most important phenomena. With an equally judicial mind, Huyghens would perhaps have decided so too, in the face of difficulties, all of which have not been cleared up even to-day. These two great men, then, each looked around in the then darkness as far as his light carried him. All beyond that was chance to each; and fate willed that Newton, whose light shone farther than his rivals, found it extend just far enough to show the entrance to the wrong way. He reaches the conclusion that we all know; one not only wrong in regard to light, but which bears pernicious results on the whole theory of heat, since light being conceded to be material, radiant heat, if affiliated to light, must be regarded as material too, and Newton's influence is so permanent, that we shall see this strange conclusion drawn by the contemporaries of Herschel from his experiments made a hundred years later.

It would seem then that the result of this unhappy corpuscular theory was more far-reaching than we commonly suppose, and that it is hardly too much to say that the whole promising movement of that age toward the true doctrine of radiant energy is not only arrested by it, but turned the other way; so that in this respect the philosophy of fifty years later is actually farther from the truth than that of Newton's predecessors, and the immense repute of Newton as a leader, on the whole so rightly earned, here leads astray others than his conscious disciples, and, it seems to me, affects men's opinions on topics which appear at first far removed from those he discussed. The adoption of phlogiston was, as we may reasonably infer, facilitated by it, and remotely Newton is perhaps also responsible in part for the doctrine of caloric a hundred years later. After him, at any rate, there is a great backward movement. We have a distinct retrogression from the ideas of Bacon and Hobbes and Boyle. Night settles in again on our subject almost as thick as in the days of the schoolmen, and there seems to be hardly an important contribution to our knowledge, in the first part of the eighteenth century, due to a physicist.

"Physics, beware of metaphysics," said Newton,—words which physicists are apt so exclusively to quote, that it seems only due to candor to observe that the most important step, perhaps, in the fifty years which followed the "Optics," came from Berkeley, who, reasoning as a metaphysician, gave us during Newton's lifetime a conception wonderfully in advance of his age. Yet the "New Theory of Vision" was generally viewed by contemporary philosophers as only an amusing paradox, while "coxcombs vanquish[ed] Berkeley with a grin;" and this contribution to science,—an exceptional if not a unique instance of a great physical generalization reached by *a priori* reasoning,—though published in 1709, remains in advance of the popular knowledge even in these closing years of the nineteenth century.

In the meantime a new error had risen among men,—a new truth, as it seemed to them,—and a thing destined to have a strong reflex action on the doctrine of radiant energy. It began with the generalization of a large class of phenomena which we now associate with the action of oxygen, then of course unknown, a generalization useful in itself, and accompanied by an explanation which was not in its origin objectionable. Let us consider, in illustration, any familiar instance of oxidation, and try to look first for what was reasonable in the eighteenth-century views of the cause of such phenomena.

A piece of dry wood has in it the power of giving out heat and light when set on fire; but after it is consumed there is left of it only inert ashes, which can give neither. Something, then, has left the wood in process of becoming ashes; virtue has gone out of it, or, as we should say, its potential energy has gone.

This is, so far, an important observation, extending over a wide range of phenomena, and, if it had presented itself to the predecessors of Newton, it would probably have been allied to the vibratory theories, and become proportionately fruitful. But to his disciples, and to chemists and others, who, without being perhaps disciples, were like all then, more or less consciously influenced by the materiality of the corpuscular theory, it appeared that this virtue also was a material emanation;—that this energy was an actual ingredient of the wood,—a crudeness of conception which seems most strange to us, but is not perhaps unaccountable in view of the then current thought.

I have said that the progress of science is not so much that of an army as of a crowd of searchers, and that a call in a false direction may be responded to, not by one only, but by the whole body. In illustration, observe that during the greater part of the entire eighteenth century, this doctrine was adopted by almost every chemist and by many physicists. It had as general an acceptance among chemists then as the kinetic theory of gases, for instance, has among physicists now, and, so far as time is any test of truth, it was tested more severely than the kinetic theory has yet been; for it was not only the lamp and guide of chemists, and to a great extent of physicists also, but it remained the time-honored and highest generalization of chemical science for over half a century, and it was accepted not so much as a conditional hypothesis, as a final guide, and a conquest for truth which should endure always. And now where is it? Dissipated so utterly from men's minds, that, to the unprofessional part of even an educated audience like this, "phlogiston," once a name to conjure with, has become an unmeaning sound.

There is no need to insist on the application of the obvious moral to hypotheses of our own day.

I have tried to recall for a moment all that "phlogiston" meant a little more than a hundred years ago, partly because it seems to me, that, though a chemical conception, physics is not blameless for it, but chiefly because before it quitted the world it appears to have returned to physics the wrong in a multiplied form, by generating an offspring specially inimical to true ideas about radiant heat, and which is represented by a yet familiar term. I mean "caloric."

This word is still used loosely as a synonym for heat, but has quite ceased to be the very definite and technical term it once was. To me it has been new to find that this so familiar word "caloric," so far as my limited search has gone, was apparently coined only toward the last quarter of the last century. It is not to be found in the earliest edition of Johnston's dictionary, and, as far as I can learn, appears first in the corresponding French form in the works of Fourcroy. It expressed an idea which was the natural sequence of the phlogiston theory, and which is another illustration that the evil which such theories do lives after them.

"Caloric" first seemingly appears, then, as a word coined by the French chemists, and meant originally to signify the unknown cause of the sensation heat, without any implication as to its nature. But words, we know, though but wise men's counters, are the money of fools; and this one very soon came to commit its users to an idea which was more likely to have had its origin in the mind of a chemist at that time than of any other,—the idea of the cause of heat as a material ingredient of the hot body; something not, it is true, having weight, but which it would have been only a slight extension of the conception, to think might one day be isolated by a higher chemical art, and exhibited in a tangible form.

We may desire to recognize the perverted truth which usually underlies error and gives it currency, and be willing to believe that even "caloric" may have had some justification for its existence; but this error certainly seems to have been almost altogether pernicious for nearly the next eighty years, and down even to our own time. With this conception as a guide to the philosophers of the last years of the eighteenth century, it is not, at any rate, surprising if we find that at the end of a hundred years from Newton the crowd seems to be still going constantly farther and farther away from its true goal.

The doctrine of caloric is, however, always recognized as an hypothesis more acceptable to chemists than to physicists, some of whom still stand out for the theories of Newton's predecessors, even through the darkest years; so that the old idea of heat, as a mode of motion, has by no means so utterly died that it does not appear here and there during the last century, and indeed, not only among philosophers, but even in a popular form.

In an old English translation of Father Regnault's compilation on physics, dated about 1730, I find, for instance, the most explicit statement of the doctrine of heat as a mode of motion. Here heat is defined (with the aid of a simile due, I believe, to Boyle) as "any Agitation whatever of the insensible parts. Thus a Nail which is drove into the Wood by the stroke of a Hammer does not appear to be hot, because its immediate parts have but one common Movement. But should the Nail cease to drive, it would acquire a sensible Heat, because its insensible Parts which receive the Motion of the Hammer now acquire an agitation every way rapid." We certainly must admit that the user of this illustration had just and clear ideas; and the interesting point here appears to be, that as Father Regnault's was not an original work, but a mere compendium or popular scientific treatise of the period, we see, if only from this instance, that the doctrine of heat as a mode of motion was not confined to the great men of an earlier or a later time, but formed a part of the common pabulum during the eighteenth century to an extent that has been forgotten.

Although Prevost gave us his most material contribution about 1790, we have, it seems to me, on the whole, little to interest us during that barren time in the history of radiant energy called the eighteenth century,—a century in which science wore the pedant's cap and gown, and her students read the poem of Creation like grammarians, for its syntax;—a century whose latter years are given up, till near its very close, to bad *a priori* theories in our subject, except in the work of two Americans; for in the general dearth at this time of experiments in radiant heat, it is a pleasure to fancy Benjamin Franklin sitting down before the fire, with a white stocking on one leg and a black one on the other, to see which leg would burn first, and to recall again how Benjamin Thompson (Count Rumford) not only weighed "caloric" literally in the balance and found it wanting, but made that memorable experiment in the Munich founderies which showed that heat was perpetually and without limit created from motion.

It was in the last years of the century, too, that he provided for the medal called by his name, and which, though to be given for researches in heat and light, has, I believe, been allotted in nearly every instance to men, who, like Leslie, Malus, Davy, Brewster, Fresnel, Melloni, Faraday, Arago, Stokes, Maxwell, and Tyndall, have contributed toward the subject of radiant energy in particular.

We observe that before this time the scientific literature of the century scarcely considers the idea even of radiant heat, still less of radiant energy; so that we have been obliged here to discuss the views of its physicists about heat in general, heat and light in most minds being then distinct entities; all the ways for pilgrims to this special shrine of truth being barred, like those in Bunyan's Pilgrim's Progress, by the two unfriendly giants who are here called Phlogiston and Caloric, so that there are few scientific pilgrims who do not pay them toll.

The last years of this century were destined to see the most remarkable experiments in heat made in the whole of the hundred; for the memoir of Rumford appeared in the Philosophical Transactions for 1798; and in the very year 1800 appeared in the same place Sir William Herschel's paper, in which he describes how he placed a thermometer in successive colors of the solar spectrum, finding the heat increase progressively from the violet to the red, and increase yet more beyond the red where there was no color or light whatever; so that there are, he observes, invisible rays as well as visible. More than that, the first outnumber the second; and these dark rays are found in the very source and fount of light itself. These dark rays can also be obtained, he observes, from a candle or a piece of non-luminous hot iron, and, what is very significant, they are found to pass through glass, and to be refracted by it like luminous ones.

And now Herschel, searching for the final verity through a series of excellent experiments, asks a question which shows that he has truth, so to speak, in his hands,—he asks himself the great question whether heat and light be occasioned by the same or different rays.

Remember the importance of this (which the querist himself fully recognized); remember, that, after long hunting in the blindfold search, he has laid hands, as we now know, on the truth herself, and then see him—let go. He decides that heat and light are not occasioned by the same rays, and we seem to see the fugitive escape from his grasp, not to be again fairly caught till the next generation.

I hardly know more remarkable papers than these of Herschel's in the Philosophical Transactions for 1800, or anything more instructive in little men's successes than in this great man's failure, which came in the moment of success. I would strongly recommend the reading of these remarkable original memoirs to any physicist who knows them only at second-hand.

One more significant lesson remains, in the effect of this on the minds of his contemporaries. Herschel's observation is to us almost a demonstration of the identity of radiant heat and light; but now, though the nineteenth century is opening, it is with the doctrine in the minds of most physicists, and perhaps of all chemists, that heat is occasioned by a certain material fluid. Phlogiston is by this time dead or dying, but Caloric is very much alive, and never more perniciously active than now, when, for instance, years after Herschel's observation, we find this cited as "demonstrating the existence of caloric;"—which was, it seems, the way it looked to a contemporary.

In the year 1804 appeared what should be a very notable book in the history of our subject, written by Sir John Leslie, whose name survives perhaps in the minds of many students chiefly in connection with the "cube" which is still called after him.

Leslie, however, ought to be remembered as a man of original genius, worthy to be mentioned with Herschel and Melloni; and his, too, is one of the books which the student may be recommended to read, at least in part, in the original; not so much for the writer's instructive experiments (which will be found in our text-books) as for his most instructive mistakes, which the text-book will probably not mention.

He began by introducing the use of the simple instrument which bears his name, and a new and more delicate heat-measurer (the differential thermometer); and with these, and concave reflectors of glass and metal, he commenced experiments in radiant heat, than which, he tells us, no part of physical science then appeared so dark, so dubious, and so neglected. It is interesting, and it marks the degree of neglect he alludes to, that his first discovery was that different substances have different radiating and absorbing powers. It gives us a vivid idea of the density of previous ignorance, that it was left to the present century to demonstrate this elementary fact, and that Leslie, in view of such discoveries, says, "I was transported at the prospect of a new world emerging to view."

Next he shows that the radiating and absorbing powers are proportional, next that cold as well as heat seems to be radiated, and next undertakes to see whether this radiant heat has any affinity to light. He then experiments in the ability of radiant heat to pass through a transparent glass, which transmits light freely, and thinks he finds that none does pass. Radiant heat with him seems to mean heat from non-luminous sources; and the ability or non-ability of this to pass through glass, is to Leslie and his successors a most crucial test, and its failure to do so a proof that this heat is not affiliated to light.

Let us pause a moment here to reflect that we are apt to unconsciously assume, while judging from our own present standpoint where past error is so plain, that the false conclusion can only be chosen by an able, earnest, conscientious seeker, after a sort of struggle. Not so. Such a man is found welcoming the false with rapture, as very truth herself.

"What then," says Leslie, "is this calorific and frigorific fluid after which we are inquiring? It is not light, it has no relation to ether, it bears no analogy to the fluids, real or imaginary, of magnetism and electricity. But why have recourse to invisible agents? *Quod petis, hic est.* It is merely the ambient AIR."

The capitals are Leslie's own, but ere we smile with superior knowledge, let us put ourselves in his place, and then we may comprehend the exultation with which he announces the identity of radiant heat and common air, for he feels that he is beginning a daring revolt against the orthodox doctrine of caloric; and so he is.

The first five years of this century are notable in the history of radiant energy, not only for the work of Leslie, and for the observation by Wollaston, Ritter, and others, of the so-called "chemical" rays beyond the violet, but for the appearance of Young's papers, reëstablishing the undulatory theory, which he indeed considered in regard to light, but which was obviously destined to affect most powerfully the theory of radiant energy in general.

We are now in the year 1804, or over a century and a quarter since the corpuscular theory was emitted, and during that time it has gradually grown to be an article of faith in a sort of scientific church, where Newton has come to be looked on as an infallible head, and his views as dogmas, about which no doubt is to be tolerated; but if we could go back to Cambridge in the year 1668, when the obscure young student, in no way conscious of his future pontificate, takes his degree (standing twenty-third on the list of graduates), we should probably find that he had already elaborated and greatly improved certain already current ideas into the undulatory theory of light, which he at any rate promulgated a few years later, and afterward, pressed with many difficulties, altered, as we now know, to an emissive one.

Probably, if we could have heard his own statement then, he would have told how sorely tried he was between these two opinions, and, while explaining to us how the wavering balance came to lean as it did, would have admitted, with the modesty proper to such a man, that there was a great deal to be said on either side. We may, at any rate, be sure that it would not be from the lips of Newton himself that we should have had this announced as a belief which was to be part of the rule of faith to any man of science.

But observe how, if science and theology look askance at each other, it is still true that some scientific men and some theologians have, at any rate, more in common than either is ready to admit; for at the beginning of this century Newton's followers, far less tolerant than their master, have made out of this modest man a scientific pontiff, and out of his diffident opinions a positive dogma, till as years go on, he comes to be cited as so infallible that a questioning of these opinions is an offense deserving excommunication.

This has grown to be the state of things in 1804, when Young, a man possessing something of Newton's own greatness, ventures to put forward some considerations to show that the undulatory theory may be the true one, after all. But the prevalent and orthodox scientific faith was still that of the material nature of light; the undulatory hypothesis was a heresy, and Young a heretic. If his great researches had been reviewed by a physicist or a brother worker, who had himself trodden the difficult path of discovery, he might have been treated at least intelligently; but then, as always, the camp-followers, who had never been at the front, shouted from a safe position in the rear to the man in the dust of the fight, that he was not proceeding according to the approved rules of tactics; then, as always, these men stood between the public and the investigator, and distributed praise or blame.

If you wish to hear how the scientific heretic should be rebuked for his folly, listen to one who never made an observation, but, having a smattering of everything books could teach about every branch of knowledge, was judged by himself and by the public to be the fittest interpreter to it, of the physical science of this day. I mean Henry Brougham, the universal critic, the future Lord Chancellor of England, of whom it was observed, that, "if he had but known a little *law*, he would have known a little of everything." He uses the then all-powerful *Edinburgh Review* for his pulpit, and from it fulminates the condemnations of the church on the innovating memoir of the heretical Young.

"This paper," he says, "contains nothing which deserves the name of experiment or discovery; and it is, in fact, destitute of every species of merit. . . . first is another lecture, containing more fancies, more blunders, more unfounded hypotheses, more gratuitous fictions . . . and all from the fertile yet fruitless brain of the eternal Dr. Young. In our second number we exposed the absurdity of this writer's 'law of interference,' as it pleases him to call one of the most incomprehensible suppositions that we remember to have met with in the history of human hypotheses."

There are whole pages of it, but this is enough; and I cite this passage among many such at command, not only as an example of the way the undulatory theory was treated at the beginning of this century in the first critical journal of Europe, but as another example of the general rule that the same thing may appear intrinsically absurd, or intrinsically reasonable, according to the year of grace in which we hear of it. The great majority, even of students of science, must take their opinions ready-made as to science in general; each knowing, so far as he can be said to know anything at first-hand, only that little corner which research has made specially his own.

The moral we can all draw, I think, for ourselves.

In spite of such criticism as this, the undulatory hypothesis of light made rapid way, and carried with it, one would now say, the necessary inference that radiant heat was due to undulations also. This was, however, no legitimate inference to those to whom radiant heat was still a fluid; and yet, in spite of all, the modern doctrine now begins to make visible progress.

A marked step is taken about 1811 by a young Frenchman, De la Roche, who deserves to be better remembered than he is, for he clearly anticipated some of Melloni's discoveries. De la Roche in particular shows that of two successive screens the second absorbs heat in a less ratio than the first; whence he, before any one else, I believe, derives the just and most important, as well as the then most novel conception, that radiant heat is of different *kinds*. He sees also, that, as a body is heated more and more, there is a gradual and continual advance not only in the amount of heat it sends out, but in the kind, so that, as the temperature still rises, the radiant heat becomes light by imperceptible gradations; and he concludes that heat and light are due to one simple agent, which, as the temperature rises yet more, appears more and more as light, or which, as the luminous radiation is absorbed, re-appears as heat. Very little of it, he observes, passes even transparent screens at low temperatures, but more and more does so as the temperature rises.

All this is a truism in 1888, but it appears admirably new as well as true in 1811; and if De la Roche had not been removed by an early death, his would have not improbably been the greatest name of the century in the history of our subject; an honor, however, which was in fact reserved for another.

The idea of the identity of light and radiant heat had by this time made such progress that the attempt to polarize the latter was made in 1818 by Berard. We have just seen in Herschel's case how the most sound experiment may lead to a wrong conclusion, if it controvert the popular view. We now have the converse of this in the fact that the zeal of those who are really in the right way may lead to unsound and inconclusive experiment; for Berard experimentally established, as it was supposed, the fact that obscure radiant heat can be polarized. So it can, but not with such means as Berard possessed, and it was not till a dozen years more that Forbes actually proved it.

At this time, however fairly we seem embarked on the paths of study which are followed to-day, and while the movement of the main body of workers is in the right direction, it is yet instructive to observe how eminent men are still spending great and conscientious labor, their object in which is to advance the cause, while the effect of it is to undo the little which has been rightly done, and to mislead those who have begun to go right.

As an instance both of this and of the superiority of modern apparatus, we may remark,—after having noticed that the ability of obscure heat to pass through glass, if completely established, would be a strong argument in favor of its kinship to light, and that De la Roche and others had indicated that it would do so (in which we now know they were right),—that at this stage, or about 1816, Sir David Brewster, the eminent physicist, made a series of experiments which showed that it would not so pass. Ten years later, in view of the importance of the theoretical conclusion, Baden Powell repeated his observations with great care, and confirmed them, announcing that the earlier experimenters were wrong, and that Brewster was right; so that here all these years of conscientious work resulted in establishing, so far as it could be established, a wholly wrong conclusion in place of a right one already gained.

It may be added, that with our present apparatus, the passage of obscure radiant heat through glass could be made convincingly evident in an experiment which need not last a single second.

We are now arrived at a time when the modern era begins; and in looking back over one hundred and fifty years, from the point of view of the experimenter himself with his own statement of the truth as he saw it, we find that the comparison of the progress of science to that of an army, which moves, perhaps with the loss of occasional men, but on the whole victoriously and in one direction, is singularly misleading; and I state this more confidently here, because there are many in this audience who did not get their knowledge of nature from books only, but who have searched for the truth themselves; and, speaking to them, may I not say that those who have so searched know that the most honest purpose and the most patient striving have not been guaranties against mistakes,—mistakes which were probably hailed at the time as successes? It was some one of the fraternity of seekers, I am sure, who said, "Show me the investigator who has never made a mistake, and I will show you one who has never made a discovery."

We have seen the whole scientific body, as regards this particular science of radiant energy, moving in a mass, in a wrong direction, for a century; we have seen that individuals in it go on their independent paths of error; and we can only wonder that an era should have come in which such a real advance is made as in ours.

That era has been brought in by the works of many, but more than by any other through the fact that, in the year 1801, there came into the world at Parma an infant who was born a physicist, as another is born a poet; nay, more; who was born, one might say, a devotee of one department of physics, that of radiant heat; being affected in his tenderest years with such a kind of precocious passion for the subject as the childish Mozart showed for music. He was ready to sacrifice everything for it; he struggled through untold difficulties, not for the sake of glory or worldly profit, but for radiant heat's sake; and when fame finally came to him, and he had the right to speak of himself, he wrote a preface to his collected researches, which is as remarkable as anything in his works. In this preface he has given us, not a summary of previous memoirs on the subject, not a table of useful factors and formulæ, not anything at all that an English or an American scientific treatise usually begins with, but the ingenuous story of his first love, of his boyish passion for this beloved mistress; and all this with a trust in us, his readers, which is beautiful in its childlike confidence in our sympathy.

I should need to abbreviate and injure in order to quote; but did ever a learned physical treatise and collections of useful tables begin like this before?

"I was born at Parma, and when I got a holiday used to go into the country the night before, and go to bed early, so as to get up before the dawn. Then I used to steal silently out of the house, and run, with bounding heart, till I got to the top of a little hill, where I used to set myself so as to look toward the East." There, he tells us, he used, in the stillness of nature, to wait the rising sun, and feel his attention rapt, less with the glorious spectacle of the morning light itself than with the sense of the mysterious heat which accompanied its beams, and brought something more necessary to our life and that of all nature than the light itself, so that the idea that not only mankind, but nature, would perish though the light continued, if this was divorced from heat, made a profound impression, he tells us, on his childish mind.

The statement that such an idea could enter with dominating force into the mind of a child will perhaps seem improbable to most. It will, however, be credible enough to some here, I have no doubt.

Is there some ornithologist present who remembers a quite infantile attraction which birds possessed for him above all the rest of the animated creation? Some chemist whose earliest recollections are of the strange and quite abnormal interest he found as a child in making experimental mixtures of every kind of accessible household fluid and solid? Some astronomer who remembers that when a very little creature not only the sight of the stars, but of any work on astronomy, even if utterly beyond his childish comprehension, had an incomprehensible attraction for him? I will not add to the list. There are, at any rate, many here who will understand Melloni when he tells how this radiant heat, commonplace to others, was wonderful to his childish thought, and wrought a charm on it such that he could not see wood burn in a fireplace, or look at a hot stove, without its drawing his mind, not to the fire or iron itself, but to the mysterious effluence which it sent.

This was the youth of genius; but let not any fancy that genius in research is to be argued from such premonitions alone, unless it can add to them that other qualification of genius which has caused it to be named the faculty of taking infinite pains. Melloni's subsequent labors justified this last definition also; but I cannot speak of them here, further than to say, that, after going over a large part of his work myself, with modern methods and with better apparatus, he seems to me the man, of all great students of our subject, who, in reference to what he accomplished, made the fewest mistakes.

Melloni is very great as an experimenter, and owes much of his success to the use of the newly invented thermopile, which is partly his own. I can here, however, speak only of his results, and of but two of these,—one generally known; the other, and the more important, singularly little known, at least in connection with him.

The first is the full recognition of the fact, partly anticipated by De la Roche, that radiant heat is of different kinds, and that the invisible emanations differ among themselves just as those of light do. Melloni not only established the fact, but invented a felicitous term for it, which did a great deal to stamp it on recognition,—the term "thermochrose," or heat-color, which helps us to remember, that, as the visible and apparently simple emanation of light is found to have its colors, so radiant heat, the invisible but apparently simple emanation, has what would be colors to an eye that could see them. This result is well known in connection with Melloni.

The other and the greater, which is not generally known as Melloni's, is the generalization that heat and light are effects of one and the same thing, and merely different manifestations of it. I translate this important statement as closely as possible from his own words. They are that

"*Light is merely a series of calorific indications sensible to the organs of sight, or vice versa, the radiations of obscure heat are veritable* INVISIBLE RADIATIONS *of light.*"

The italics and the capitals are Melloni's own. He wishes to have no ambiguity about his announcement behind which he may take shelter; and he had so firm a grasp of the great principle, that, when his first attempts to observe the heat of the moon failed, he persevered, because this principle assured him that where there was light there must be heat. This statement was made in 1843, and ought, I think, to insure to Melloni the honor of being first to thus distinctly announce this great generalization.

The announcement passed apparently unnoticed, in spite of his acknowledged authority; and the general belief not merely in different entities in the spectrum, but in a material caloric, continued as strong as ever. If you want to see what a hold on life error has, and how hard it dies, turn to the article "heat," in the eighth edition of the "Encyclopædia Britannica," where you will find the old doctrine of caloric still in possession of the field in 1853; and still later, in the generally excellent "English Encyclopædia" (edition of 1867), the doctrine of caloric is, on the whole, preferred to the undulatory hypothesis. It is very probable that a searcher might find many traces of it yet lingering among us; so that Giant Caloric is not, perhaps, even yet quite dead, though certainly grown so crazy and stiff in the joints, that he can now harm pilgrims no more.

So far as I know, no physicist of eminence reasserted Melloni's principle with equal emphasis, till J. W. Draper, in 1872. Only sixteen years ago, or in 1872, it was almost universally believed that there were three different entities in the spectrum, represented by actinic, luminous and thermal rays. Draper remarks that a ray consists solely of ethereal vibrations whose lost *vis viva* may produce either heat or chemical change. He uses Descartes' analogy of the vibration of the air, and sound; but he makes no mention either of Descartes or of Melloni, and speaks of the principle as leading to a modification of views then "universally" held. Since that time the theory has made such rapid progress, that, though some of the older men in England and on the European continent have not welcomed it, its adoption among all physicists of note may be said to be now universal, and a new era in our history begins with it. I mean with the recognition that there is one radiant energy which appears to us as "actinic," or "luminous" or "thermal" radiation, according to the way we observe it. Heat and light, then, are not things in themselves, but whether different sensations in our own bodies, or different effects in other bodies, are merely effects of this mysterious thing we call radiant energy, without doing more in this than give a name to the ignorance which still hangs over the ultimate cause.

I am coming down dangerously near our own time, for one who would be impartial in dealing with names of those still living. In such a brief review of this century's study of radiant energy in other forms than light, it has been necessary to pass without mention the labors of such men as Pouillet and Becquerel in France, of Tyndall in England, and of Henry in America. It has been necessary to omit all mention of those who have advanced the knowledge of radiant energy as light, or I should have had to speak of labors so diverse as those of Fraunhofer, of Kirchhoff, of Fresnel, of Stokes, of Lockyer, of Janssen, and many more. I have made no mention, in the instructive history of error, of many celebrated experimental researches; in particular of such a problem as the measurement of solar heat, great in importance, but apparently most simple in solution, yet which has now been carried on from generation to generation, each experimenter materially altering the result of his predecessor, and where our successors will probably correct our own results in turn. I have not spoken of certain purely experimental investigations, like those of Dulong and Petit, which have involved immense and conscientious labor, and have apparently rightly earned the name of "classic" from one generation, only to be recognized by the next as leading to untrustworthy results, and leaving the work to be done again with new methods, guided by new principles.

In these instances, painstaking experiments have proved insufficient, less from want of skill in the investigator than from his ignorance of principles not established in time to enable him to interpret his experiments; but, if there were opportunity, it would be profitable to show how inexplicably sometimes error flourishes, grows, and maintains an apparently healthy appearance of truth, without having any root whatever. Perhaps I may cite one instance of this last from my own experience.

About ten years ago it was generally believed that the earth's atmosphere acted exactly the part of the glass in a hotbed, and that it kept the planet warm by exerting a specially powerful absorption on all infra-red rays. I had been reared in the orthodox scientific church, of which I am happy to be still a member; but I had acquired perhaps an almost undue respect, not only for her doctrines, but for her least sayings. Accordingly, when my own experiments did not agree with the received statement, I concluded that my experiments must be wrong, and made them all over again, till spring, summer, autumn and winter had passed, each season giving its own testimony; and this for successive years. The final conclusion was irresistible, that the universal statement of this alleged well-known fact, inexplicable as this might seem, in so simple a matter, was directly contradicted by experiment. I had some natural curiosity to find how every one knew this to be a fact; but search only showed the same statement (that the earth's atmosphere absorbed dark heat like glass) repeated everywhere, with absolutely nowhere any observation or evidence whatever to prove it, but each writer quoting from an earlier one, till I was almost ready to believe it a dogma superior to reason, and resting on the well-known "*Quod semper, quod ubique, quod ab omnibus, creditum est.*" Finally I appear to have found its source in the writings of Fourier, who, alluding to De Saussure's experiments (which showed that dark heat passed with comparative difficulty through glass) observes that if the earth's atmosphere were solid, it would act as the glass does. Fourier simply takes this (in which he is wrong) for granted; but, as he is an authority on the theory of heat, his words are repeated without criticism, first by Poisson, then by others, and then in the text-books; and, the statement gaining weight by age, it comes to be believed absolutely, on no evidence whatever, for the next sixty years, that our atmosphere is a powerful absorber of precisely those rays which it most freely transmits.

The question of fact here, though important, is, I think, quite secondary to the query it raises as to the possible unsuspected influence of mere tradition in science, when we do not recognize it as such. Now, members of any church are doubtless consistent in believing in traditions, if they believe that these are presented to them by an infallible guide; but are we, who have no infallible guide, quite safe in believing all we do, from our fond persuasion that in the scientific body mere tradition has no weight?

In even this brief sketch of the growth of the doctrine of radiant energy, we have perhaps seen that the history of the progress of this department of science is little else than a chapter in that larger history of human error which is still to be written, and which, it is safe to say, would include illustrations from other branches of science, as well as my own. But —and here I ask pardon if I speak of myself—I have been led to review the labors of other searchers from this standpoint, because I had first learned, out of personal experience, that the greatest care was no certain guaranty of final accuracy; that to labor in the search for a truth with such endless pains as a man might bestow if his own salvation were in question, did not necessarily bring the truth; and because, seeking to see whether this were the lot of other and greater men, I have found that it was, and that, though no one was altogether forsaken of the truth he sought, or, on the whole review of his life as a seeker, but might believe he had advanced her cause, yet there was no absolute criterion by which it could be told at the time, whether, when after long waiting, there came in view what seemed once more her beautiful face, it might not possibly prove, after all, the mockery of error; and, doubtless, appeal might be made to the experience of many investigators here with the question, "Is it not so?"

What then? Shall we admit that truth is only to be surely found under the guidance of an infallible church? If there be such a church, yes! Let us, however, remember that the church of science is not such a one, and be ready to face all the consequences of the knowledge that her truths are put forward by her as provisional only, and that her most faithful children are welcome to disprove them.

What then, again? Shall we say that the knowledge of truth is not advancing? It is advancing, and never so fast as to-day; but the steps of its advance are set on past errors, and the new truths become such stepping-stones in turn.

To say that what are truths to one generation are errors to the next, or that truth and error are but different aspects of the same thing to our poor human nature, may be to utter truisms; but truisms which one has verified for one's self out of a personal experience are apt to have a special value to the owner; and these lead, at any rate, to the natural question, "Where is, then, the evidence that *we* are advancing in reality, and not in our own imagination?"

There are many here who will no doubt heartily subscribe to the belief that there is no absolute criterion of truth for the individual, and admit that there is no positive guaranty that we, with this whole generation of scientific men, may not, like our predecessors, at times go the wrong way in a body, yet who believe as certainly that science as a whole, and this branch of it in particular, is actually advancing with hitherto unknown rapidity. In asking to be included in this number, let me add that to me the criterion of this advance is not in any ratiocination, not in any *a priori* truth, still less in the dictum of any authority, but in the undoubted observation that our doctrine of radiant energy is reaching out in every direction, and proving itself by the equally undoubted fact that through its aid nature obeys us more and more; proving itself by such material evidence as is found in the electric lights in our streets, and in a thousand such ways which I need not pause to enumerate.

And here I might end, hoping that there may be some lessons for us in the history of what has been said. I will venture to ask attention to but one. It is that in these days, when the advantage of organization is so fully recognized, when there is a well-founded hope that by coöperation among scientific men knowledge may be more rapidly increased, and when not only in the great scientific departments of government but everywhere, there is a tendency to the formation of the divisions of a sort of scientific army, not to say of a scientific church—that at such a time we should yet remember, that, however rapidly science changes, human nature remains much the same; and (while we are uttering truisms) let us venture to repeat that there is a very great deal of this "human nature" even in the scientific man, whose best type is one nearly as independent as nature itself, and one which will not always work best at the word of command. Let him then never forget that the history of science, scarcely less than of theology, warns him of the tendency of authority to exceed its proper sphere, and from without it, to define belief, and to impose obedience to doctrine.

Finally, if, turning to the future, I were asked what I thought were the next great steps to be taken in the study of radiant heat, I should feel unwilling to attempt to look more than a very little way in advance. Immediately before us, however, there is one great problem waiting solution. I mean the relation between temperature and radiation; for we know almost nothing of this, where knowledge would give new insight into almost every operation of nature (nearly every one of which is accompanied by the radiation or reception of heat), and would enable us to answer inquiries now put to physicists in vain by every department of science, from that of the naturalist as to the enigma of the brief radiation of the glowworm, to that of the geologist who asks as to the number of million years required for the cooling of a world.

When, however, we begin to go beyond the points which seem, like this, to invite our very next steps in advance, we cannot venture to prophesy, and must content ourselves with the knowledge that through our study we are beginning to apprehend the full meaning of one of the early great ones of science, who described man as the meeting point of two infinities. That there is an infinity of space above him, man has long known, but that there is another absolute infinity, and what the possibilities are which lie in the infinitesimals of space, he is but beginning to realize. The secular movements, whose accomplishment demands more than a million years of time, he has already considered; but of the consequences which may result from a more careful study of actions occurring in the infinitesimals of time, and whose whole duration may be far less than the millionth of a second, he has hardly even yet begun to think; and these are but little portions of the ungarnered field of research, open to the student of that radiant energy which sustains, with our own being, that of all animated nature, of which humanity is but a part.

If there be any students of nature here, who, feeling drawn to labor in this great field of hers, still doubt whether there is yet room, surely it may be said to them, "Yes, just as much room as ever, as much room as the whole earth offered to the first man;" for everything that has been done in the past is, I believe, as nothing to what remains before us, and that field is simply unbounded. The days of hardest trial and incessant bewildering error in which your elders have wrought, seem over. You "in happier ages born," you of the younger and the coming race, who have a mind to enter in and possess it, may, as the last word here, be bidden to indulge in an equally unbounded hope.

[FROM THE AMERICAN JOURNAL OF SCIENCE, VOL. XXXVIII, AUGUST, 1889.]

ON THE OBSERVATION OF SUDDEN PHENOMENA.

BY S. P. LANGLEY.

[FROM THE AMERICAN JOURNAL OF SCIENCE, VOL. XXXVIII, AUGUST, 1889.]

ART. XI.—*On the Observation of Sudden Phenomena;* by S. P. LANGLEY.

[Read before the Philosophical Society of Washington, March 2, 1889.]

BY a sudden phenomenon is here meant one of that large class where the occurrence is awaited without the observer's previous knowledge of its exact instant, and of which familiar examples may be found in the bursting of a rocket, the appearance of a meteor, or the emergence of a star from behind the moon. A great part of all the phenomena of daily life, as well as of scientific observation, are of this kind, though the importance of a special instance of another class, (I refer to the gradual and foreseen approach of a star to a wire), has drawn to this latter such particular attention that we are apt to think of it only when "personal error" is in question.

When in an observatory, we study the means taken to record the precise time of the transit of a star, we find that the precision of modern apparatus has reduced the error which we may expect in almost any part of the mechanism to an extremely minute amount, which may be calculated to the fractional part of the one hundredth of a second. I say "almost" for, as we are all aware, there is one notable exception, at least until photography can be made to intervene. The human brain and nerves, and behind these the inscrutable processes of the will, themselves form an inevitable link in the chain of apparatus of observation, and here an error may and does arise enormously greater than that of all the rest put together.

We all know that this error varies with the individual and the occasion. It is most constant in the experienced observer, but even in his case it varies with the daily accidents of the human organism, and even with him it is presumably constant only for the particular observation to which the experience applies. There is not even a presumption, I think, that the personal equation belonging to an experienced transit observer would apply to the same person's notation of the occultation or emergence of a star, and still less, if possible, to any phenomenon outside his ordinary professional experience; for we must, of course, recognize that we carry this fallibility with us in every act of life, and that it is just as present when we attempt to determine the instant at which a race horse passes the winning post, as when we seek to note the particular hundredth of a second at which a star passes the wire.

The very words "personal equation" imply that the errors due to this fallibility can be ascertained and allowed for, and may lead us to think (if we think carelessly) that there is a personal equation always ascertainable; whereas, as we in fact know, it is only possible to apply the correction where long habit has settled the amount of error to be expected with regard to some one special phenomenon.

The number of devices for obtaining and correcting the personal equation, even in the special case of meridian observation, is, as those who have studied the subject know, surprisingly great. I think I have myself examined more than fifty such, and with hardly an exception they all exhibit variations on one idea—the idea, that is, that the error must have been committed *first;* the committing of the error being assumed to be an inevitable necessity for which subsequent correction is to be made.

I have thought, then, that it might be interesting if I were to ask you to consider with me what may seem at first the somewhat paradoxical suggestion, that means may be found by which any individual, skilled or ignorant, may make, not only meridian observations, but an observation of any sudden visible event, of whatsoever nature, so accurately that we need apply no correction, because the precision may be, if not absolute, at least such that no correction will in ordinary practice be needed. I may deceive myself in thinking that what I have to suggest involves a novel idea, but I am led to suppose so from the fact that I have met no application of it in a somewhat extended reading on this point.

Let me first remark that while such error as that in question doubtless belongs to all the senses in some degree, we are at this moment considering it in connection with the sense of sight only.

When we see anything in motion (let us suppose for instance a passing train on the railroad) we have the well-known facts that—

First. An instantaneous photograph is made by the optic lens upon the retina, there being a picture formed there, which is perfectly distinct, but which fades out upon the retinal plate in from one-tenth to one-quarter of a second, while the perception of this image is under ordinary circumstances * sensibly instantaneous; but—

Second. Nerves convey the distinct impression of every part of the picture to the brain, and it is here, if we have to act on this impression, that a certain time is lost, not only in the carrying of the message along one set of nerves and the bringing back the answer on the other, but in the decision that is being made by that unknown and inner self, which appears to us to exert here a more or less conscious act of will.

In the case of a sudden and startling event, the time elapsed may be almost indefinitely great; and in some cases, probably several entire seconds may pass without the consciousness of the observer. A very imperfectly appreciated interval must occur in all cases, for what we have just said applies to every event of our daily lives, and the professional observation is only a particular instance of it.

Now, I ask your attention to the practical instantaneity of the *formation* of this visual picture, which is known to be obtained where the duration of the phenomenon to be observed is much less than the one thousand-millionth of a second, while where we have every reason to believe that the actual formation of the image on the retina under known ordinary conditions requires a time of like order.

We may say, then, that the casting of a picture on the retina is instantaneous. It is its fading out that requires time, and it is while this fading out takes place, and even long after it, that the work of perception, decision, and action is going on behind the retinal curtain in the chambers of the brain. Notice then, that while to determine *when* a phenomenon occurs may require, under some circumstances, several seconds, and under all ordinary circumstances a notable fraction of a second, to determine *where* it occurs requires (sensibly) no time at all, for one single impression remains on the retina long enough to obtain full recognition and to be reproduced by processes of memory.

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* The writer's observations (this Journal, Nov., 1888) show that appreciable time is required for perception of the retinal impression, with certain excessively faint lights; but these are not here in question.

I can make my meaning clearer, perhaps, by using the same specific instance as before. Let us suppose that an accident to a passenger on the passing train is the phenomenon, the time of whose occurrence is to be noted, and that this accident is seen from a room in which there are two windows looking on the track. We must have seen the accident, if it be instantaneous, either through the first window or the second. If we had been led to anticipate that we should be called upon to say through which window we saw it, I think we may all admit that there would be no discrepancy on this point between different observers, for in this case we are considering only the element of position, and the element of time does not directly enter at all, so that observers in the same position who had been bidden to note through which window they saw it would all agree on this point.

Now a connection can here obviously be established between the place and the time, from which to infer the latter, if we are granted the knowledge of two facts: the time at which the carriage could have first come into view from the first window, and the time at which it must have passed out of view behind the second; for if we suppose the speed of the train to have been uniform, we have the means of deciding the fraction of the time when we know the fraction of space. Here then, as in the case of a common clock or chronograph, or any device where time and space are proportional, we can infer the latter from the former; only let it be observed that we here need no recording apparatus. What we use is the memory of where the event occurred; in other words, we recall the impression on the retinal screen and have no need to bring into use what we may call the time-perception apparatus of the brain which lies behind it; *nor do we in fact need that the object of our observation shall be really in motion, but only that it shall be made to appear to be so.*

This last point is all important, and what I ask your attention to is an experiment heretofore, I think, untried, and which is perhaps a novel application of the fundamental horological idea that time and space must be made proportional, for it seems to me it must be theoretically possible, not only in the case of the clock or the chronograph, but always, to so connect the former with the latter that the essential task of the time observer is only to say *where* any visible event apparently occurred, and then let some mechanism outside of himself say when.

That at least is the idea, and if it has, as I hope, been clearly apprehended by you, I will now ask your attention to a working plan for carrying it out. Numerous different devices have been under my consideration. I will take one which is primarily designed for the observation of any celestial phenomenon, though it could very well be adapted to terrestrial ones; and in order to fix our ideas, I will suppose that we have an event which we know the approximate time of, but which may burst upon us at some fraction of a second which we want to determine. I will assume (merely to fix our thoughts) that we wish to note the time at which a star emerges from behind the dark body of the moon, with an accuracy which ensures us that we have not made an error so great as one-twentieth of a second.

You see I hold in my hand a peculiar eye-piece, which has been made to observe this or any other terrestrial or celestial phenomenon of sudden occurrence. It can also be used for meridian observation, but its special field seems to lie in noting an event where no ordinary correction for personal equation is applicable. This event may be anything celestial or terrestrial, from the entrance of Venus on the disc of the sun, to the explosion of a mine; but, for the purpose of illustration merely, let us take it to be the sudden appearance of the star.

On looking into the telescope we see, in the first place, two prominent wires crossing each other at right angles, dividing the field of view into four quadrants. Now, by a simple mechanism, which I shall shortly explain, any object that our telescope is directed on—any fixed star for example—seems to be revolving in the field, passing successively through the first, second, third and fourth quadrants. If the star is hidden the mechanism is working just the same, and when the star appears it must evidently first be seen in some particular one of these four quadrants, and experience shows that we shall have no difficulty in telling in which one. The mechanism itself has recorded for us by an electric contact the limiting instant between which it is possible to see the beginning and the end of the cycle during which revolution may be supposed to be made. It is not necessary that this cycle should last just a second; but supposing it (still for illustration only) to be a second, if it was seen in the first quadrant, it was seen in the first quarter of the second; if it was seen in the second quadrant; some time in the second quarter of the second; in the third, in the third quarter; in the fourth, in the final quarter. All that we have to do in this case is to know in which second it occurred; for the quarter of a second we may say is noted for us by the purely automatic action of the optic lens and retina, since the first image on the retina must be that of the star as seen in some particular one of the four quadrants.

Going a little farther, we will now suppose each of the four quadrants, which in turn correspond to quarter seconds, to be divided into five parts, so that the whole circle is divided into twenty. All the observer has to say is in which quadrant and in which subdivision of the quadrant the star appears, to say in which twentieth of the second (or other brief cycle) it emerged.

The reticule I have just described is fixed in the focus of the eye-piece and does not revolve. What does revolve is a minute double prism of total reflection just before the reticule, the middle of whose reflecting face lies in the optical axis, and by whose means this axis is twice broken at a right angle, so that when the telescope is directed on a star the image of the star is not seen at the center of the field, but on one side of it. If the prism is revolved, the star must appear to revolve in a circle whose radius is nearly that of the side of the prism.

The little prism is turned by a small piece of watch-work, but it is not at all necessary that this should be exact, since all we demand is that the rate shall be constant during a second or so—a condition easily secured with the most ordinary mechanism.

The sketch and the apparatus exhibited sufficiently indicate, I think, the simple means by which this is brought about.

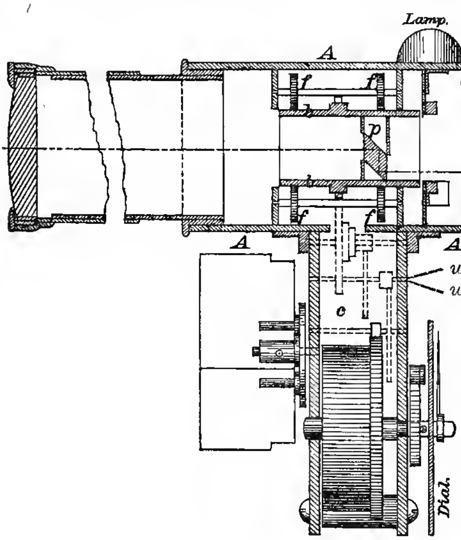

FIG. I. SECTION.
½ FULL SIZE

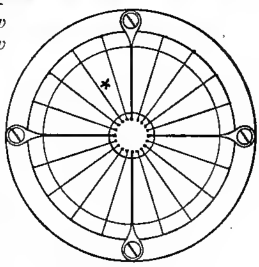

FIG. 2. DIAPHRAGM.
FULL SIZE.

S. P. LANGLEY. INVT

Figure 1 is a section one-half full size. A A is the outer tube, which can be fitted, if desired, into the eye end of a telescope. *b b* is the inner tube, resting on friction wheels *f f*, revolved by the clock-work *c c* about once a second, and recording the time at which a key in the observer's hand may be pressed to indicate the particular second. This record may be made electrically by the wires *w w* on a chronograph, or more simply and directly on a little attached dial like that of a recording stop-watch.

p p is the prism of double total reflection, *r'r* is the position of the fixed reticule (shown independently as it appears to the observer and of full size in figure 2).

e e are the lenses of a positive eye-piece. The lamp is for giving wire illumination, if desired, when a telescope is employed.

Field illumination is readily obtained otherwise by making the diaphragm in which the prism *p p* is set of translucent material.

Finally it should be remarked that on removing the eyepiece events may be observed without using any telescope. In this, its simplest form, the chronograph may also be dispensed with, and the record of the second made on an attached stop-watch dial and the instrument may thus carry its own complete recording apparatus and be more portable than an ordinary opera-glass.

I have not found opportunity to use this apparatus on the moon or occultations, but I have, what is possibly more to the purpose now, tried it on an artificial star, the instant of whose appearance and disappearance was independently recorded on a chronograph by an electrical contact. Different observers, entirely unskilled and ignorant in the use of the instrument, were invited to look into it and to determine the quadrant and section in which the star appeared and disappeared.

I have momentarily mislaid my notes containing in full detail the results of four observers but I can summarize them approximately by saying that after being simply told what to note; the average probable error (that is, for any single observation) was rather less than one-twentieth of a second. As far as I can judge from the limited number of instances, the younger the observer the better the observation. The worst of the observers (the oldest), however, had a probable error considerably less than one-tenth of a second; the youngest, a probable error of something like one-fortieth of a second, which implies, as you will observe, that he not only readily noted the quadrant and the subdivisions of the quadrant, but, also as a rule, even the part of the subdivision in which the star was first seen. None of these observers had so much as one hour's practice.

The plan in question is easily adapted to meridian observations, but for these we have numerous plans for correcting personal equation, and the writer may also direct attention to the fact of the existence of a distinct device (this Journal, July, 1877) which practically eliminates the personal error in the very act of a transit observation. It is more elaborate than the present one, which is so simple that it may be useful even in longitude work with the transit, though its proper field seems to be the observation of sudden events; but, to whatever purpose it is applied, I beg leave to present it to your attention less for any interest that attaches to the particular mechanism exhibited than as an illustration of a principle which seems to me to have not been employed before, at least in this way, and which I trust may have useful applications.

MEMOIR

OF

GEORGE BROWN GOODE.

1851-1896.

BY

S. P. LANGLEY.

READ BEFORE THE NATIONAL ACADEMY, APRIL 21, 1897.

WASHINGTON, D. C.
JUDD & DETWEILER, PRINTERS
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BIOGRAPHICAL MEMOIR OF GEORGE BROWN GOODE.

George Brown Goode was born at New Albany, Indiana, on February 13, 1851, and died at his home in Washington on September 6, 1896, after a life of forty-five years, than which few human lives have ever been better filled.

In those years he won the warm affection of a wide circle of friends and the trust and confidence of a multitude of subordinates in the position to which his own abilities had carried him. He interested himself and interested others in ever-widening circles of research, and such varied work that it seemed to those who knew what he was doing, incomprehensible that one man could accomplish so much in one single life; and when this came to an end its cessation was like the loss of a part of themselves to those who knew him best, by whom he is remembered with an affection which men rarely gain from one another.

He was the son of Francis Collier Goode and Sarah Woodruff Crane. The Goode family trace their ancestry in this country to John Goode, of Whitby, who settled in Virginia prior to 1661.*

While still settled in Virginia, many members of the Goode family went to the South and West to do pioneer work in building up villages and towns on what was then the outskirt of civilization.

Dr. Goode's father, Francis Collier Goode, was born in Waynesville, Ohio, and was a merchant in Ohio and Indiana. In 1857 he retired from business, removing to Amenia, New York; subsequently to Middletown, Connecticut, and later to Arlington,

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*The history of this family has been carefully traced by Dr. Goode in "Virginia Cousins: A Study of the Ancestry and Posterity of John Goode, of Whitby, a Virginia Colonist of the Seventeenth Century, with notes upon related families, a key to southern genealogy and a history of the English surname Gode, Goad, Goode, or Good from 1148 to 1887, by G. Brown Goode, with a preface by R. A. Brock, Secretary of the Virginia and Southern Historical Societies. Richmond, Virginia, J. W. Randolph & English," MDCCCLXXXVII.

Florida, and occasionally spent winters in the Bermudas, Tennessee, North Carolina, Virginia, and Washington city.

His mother, Sarah Woodruff Crane, was a descendant of Jasper Crane, who came to New England during the first ten years of the first settlement, and was one of the pioneers of Newark, New Jersey.

Dr. Goode was thus of sturdy American parentage on both sides, numbering among his ancestors the founders of the Virginia, Massachusetts, Connecticut, and New Jersey colonies. The family was singularly free from foreign mixture, not 10 per cent. of the marriages among the numerous descendants having been with persons whose ancestors came to America later than 1725.*

He passed his early childhood in Cincinnati and his later childhood and early youth in Amenia, New York, where he was prepared for college by private tutors. His father was a man of studious habits and not devoid of an interest in science. He had assembled in his library a set of the Smithsonian Reports, which young Goode read as a boy. It was through these volumes that he was first attracted to science and to the Smithsonian Institution, his boyish ambition being to become connected with it and to study under Professor Baird.

He entered Wesleyan University at Middletown, Connecticut, in 1866, and was graduated in 1870. Although scarcely more than fifteen when he entered college and a little over nineteen years of age at the time of his graduation, being the youngest member of the class, his work in the studies of the natural history group was so satisfactory as to attract the favorable notice of his teachers. The years at Middletown foreshadowed the strong love for nature, the museum interest, ability in classification, and even the literary talent, which were the distinguishing features of all Dr. Goode's later career.

When he went to college his father removed to Middletown and became a neighbor to Orange Judd, the pioneer of agricultural journalism in this country and closely identified with the advancement of scientific agriculture. There sprang up between the daughter of Mr. Judd and young Goode a friendship which ripened into love and resulted in their marriage, of which I speak here because Dr. Goode himself felt that the friendship with

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* "Virginia Cousins," p. xiv.

Mr. Judd, thus brought about through his daughter, had the largest share in determining his future career. The two young people had similar tastes in natural history and outdoor life. As early as 1869 Dr. Goode commenced to record in the " College Argus " and the " College Review " his outdoor rambles. He was at this time a young man of stout frame and vigorous health, engaging in all of the athletic sports known to college students of that day.

In 1870 he entered Harvard University as a post-graduate student under Professor Louis Agassiz, whose genial influence he glowingly describes in his youthful letters.

Mr. Judd had presented to Wesleyan University a building, known as the Orange Judd Hall of Natural Science. This building was in progress of erection during Mr. Goode's student years and was dedicated in the commencement week of 1871. " Before that time," says Professor Rice, " the natural history collections of Wesleyan University were scattered in several buildings, very imperfectly labeled and arranged, and mostly inaccessible to students or visitors. The spacious rooms in Judd Hall first gave the opportunity to arrange and display these collections in such manner as to give them the dignity of a museum." The work which Dr. Goode had done while a student under Professor Agassiz caused an invitation to be extended to him to undertake the arrangement of this collection, and in 1871, when but a little over twenty, he was given the title of Curator of the Museum, and undertook the installation of the collections. It was in this work that he "first showed that genius for museum administration which he was destined afterward to display in the larger field." He retained his official connection with Middletown until 1877, although the greater part of these years was spent either in Washington or in the field. During a portion of this time, although absent from Middletown, he received a salary from Wesleyan University and was allowed in exchange to send to the Museum duplicates of natural history specimens in the Smithsonian Institution, as well as the duplicates of the collections which he made. He always retained a strong feeling of affection for his alma mater, and founded the Goode prize, intended to stimulate an interest in biologic studies. He was one of the editors of the 1873 and 1883 editions of the Alumni Record of Wesleyan University, and received the honorary degree of Doctor of Laws from that institution in 1893.

Dr. Goode's mother died in his infancy, and he found in his father's second wife an affectionate and sympathetic helper, who was a strong believer in the possibility of his future scientific career. To her he owed his introduction to Professor Baird, whom he first saw at Eastport, Maine, in 1872, and this meeting was the turning point of his professional life. Through it he not only got the larger opportunities for natural history work afforded by the Fish Commission and the Smithsonian Institution, but Professor Baird singled him out almost from the first as his chief pupil, his intimate friend, his confidential adviser, and his assistant in all the natural history work in which he was engaged. The splendid advantages which Professor Baird accorded his young friend were repaid by an intense devotion.

Mr. Goode said once that he could lay down his life for such a man, and indeed he almost did so, for his originally robust health was impaired by this devotion to Professor Baird's service, particularly at the Philadelphia Exposition of 1876, which he left invalided, and the effects of his overwork in which left him a weaker man through his after life. The death of Professor Baird in 1887 affected him so deeply that it was not until 1895 that he was once heard to say that he had but just recovered from the loss.

He became in 1872 a volunteer in the United States Fish Commission, the year after the organization of that bureau, and he continued this work, making collections in 1872 at Eastport, Maine, in 1873 in Casco bay, and in 1874 at Noank, on Long Island sound. The years from 1872 to 1878 show collections of fishes made by him at the points named, as well as in Bermuda, Florida, Connecticut, and other places. Nearly twenty papers and articles relating to the Fish Commission and to fisheries appeared from his pen during the first four years of this voluntary association with the Fish Commission. He was interested not only in the scientific side of ichthyological work, but devoted great attention to the economic side. It was in 1877 that he found his first specimen of a deep-sea fish and laid the foundation of the studies which culminated in the splendid memoir on "Oceanic Ichthyology" by himself and Dr. Bean. During these years with Professor Baird he became experienced in all the work of the Fish Commission, and upon his death was appointed Commissioner of Fisheries by the President. The position up to this time had been an honorary one, but Mr. Goode informed President Cleveland that the work had grown to such an extent that it was not possible for any person who was actively engaged in the Smithsonian Institution or elsewhere to continue it. President Cleveland urged him several times to permanently accept the position of Commissioner of Fisheries, and the Committee on Appropriations of Congress had provided a salary which was larger than the one which Mr. Goode was receiving or ever did receive, but he resolutely declined, asserting that his life's ambition had been to become associated with the Smithsonian Institution; that his heart was in the Museum, and that he could not give it up. As related to his work in the Fish Commission, the facts may be mentioned that in 1877 he was employed by the Department of State on statistical work in connection with the Halifax Commission, and in 1879 and 1880 he was in charge of the Fisheries Division of the Tenth Census. His administrative abilities were strongly brought out in the organization of this work. Professor Henry F. Osborn describes his method as follows: "Special agents were sent out to every part of the coast and to the Great Lakes to gather information. Goode worked at it himself on Cape Cod and manifested the same enthusiasm as in every other piece of work he took up. He interested himself in getting together a collection representing the methods of the fisheries and the habits of the fishermen. Neglecting neither the most trivial nor important objects, branching out into every collateral matter, he showed his grasp both of principles and of details." He was United States Commissioner to the Internationale Fischerei Ausstellung in 1880 at Berlin and to the International Fisheries Exposition held at London in 1883. From circular order No. 139, issued by Commander J. J. Brice, United States Commissioner of Fish and Fisheries, I extract the following sentences: "Dr. Goode is best known for his researches and publications on the fishes and fisheries of the United States, on which subjects he came to be recognized as the leading authority. . . . He has been one of the most fruitful and valued contributors to the reports and bulletins of the Fish Commission, and in his death the fishing interests of the country have sustained a severe loss."

As I have before said, his connection with the Smithsonian

Institution followed shortly after the acquaintance with Professor Baird, who invited him to spend the winter of 1873 in Washington for the purpose of arranging the ichthyological specimens and with the understanding that as a payment for this service he was to be allowed to select duplicates for the museum at Middletown. At that time he had the title of Assistant Curator, which was later changed to Curator, and, although the relations to Middletown continued, the ties with the Institution were becoming stronger and stronger. He now met Professor Henry for the first time, and became one of the small coterie of Smithsonian men who at that time lived in the Smithsonian building and formed a part of the hospitable household which Professor Henry maintained. In these early days the staff was an extremely small one, being only thirteen persons, including honorary collaborators and subordinates. Dr. Goode threw himself into this work with uncalculating devotion. Professor Baird's duties were becoming more and more numerous, and after he became Secretary of the Institution Dr. Goode took the Museum work upon his willing shoulders. In 1881, when the new Museum building was completed and the United States National Museum really organized, Mr. Goode, then thirty years of age, was made Assistant Director. In that year he prepared a circular, known as Circular No. 1 of the National Museum, which set forth a scheme of administration for the Museum so comprehensive in its scope, so exact in its details, so practical in its ideas that it is with but few modifications still the guide for the Museum staff. On January 12, 1887, Professor Baird, whose health was then failing, appointed Mr. Goode as Assistant Secretary of the Smithsonian Institution in charge of the National Museum, and from that time until his death he had the fullest charge of the entire administration of the Museum.

It is hard to say whether Mr. Goode was best known as a museum director or a naturalist. I of course had more occasion to see his work from the administrative side. It would be impossible to understand his success in this field without thinking of the character of the man, and here I may repeat what I have said elsewhere, that if there was one quality more than another which formed the basis of his character it was sincerity—a sincerity which was the ground of a trust and confidence such as could be instinctively given even from the first only to an absolutely loyal and truthful nature.

I do not know whether a power of reading character is more intuitive or acquired, but at any rate, without it men may be governed, but not in harmony, and must be driven rather than led. Dr. Goode was in this sense a leader, quite apart from his scientific competence. Every member of the force he controlled, not only among his scientific associates, but down to the humblest employés of the Museum, was an individual to him, with traits of character which were his own and not another's, and which were recognized in all dealings, and in this I think he was peculiar, for I have known no man who seemed to possess this sympathetic insight in such a degree, and certainly it was one of the sources of his strength.

I shall have given, however, a wrong idea of him if I leave any one under the impression that this sympathy led to weakness of rule. He knew how to say "no," and said it as often as any other, and would reprehend, where occasion called, in terms the plainest and most uncompromising a man could use, speaking so when he thought it necessary, even to those whose association was voluntary, but who somehow were not alienated as they would have been by such censure from another. "He often refused me what I most wanted," said one of his staff to me; "but I never went to sleep without having in my own mind forgiven him."

I have spoken of some of the moral qualities which made all rely upon him and which were the foundation of his ability to deal with men. To them was joined that scientific knowledge without which he could not have been a Museum administrator; but even with this knowledge he could not have been what he was, except from the fact that he loved the museum and its administration above every other pursuit, even, I think, above his own special branch of biological science. He was perhaps a man of the widest interests I have ever known, so that whatever he was speaking of at any moment seemed to be the thing he knew best. It was often hard to say, then, what love predominated; but I think that he had, on the whole, no pleasure greater than that in his Museum administration, and that, apart from his family interests and joys, this was the deepest love of all. He refused advantageous offers to leave it, though I ought to grate-fully add, that here his knowledge of my reliance upon him and his unselfish desire to aid me were also among his determining motives in remaining. They were natural ones in such a man.

What were the results of this devotion may be comprehensively seen in the statement that in the year in which he was first enrolled among the officers of the Museum the entries of collections numbered less than two hundred thousand, and the staff, including honorary collaborators and all subordinates, thirteen persons, and by comparing these early conditions with what they became under his subsequent management.

Professor Baird at the first was an active manager, but from the time that he became Secretary of the Institution he devolved more and more of the Museum duties on Dr. Goode, who for nine years preceding his death was in practically entire charge of it. It is strictly within the truth, then, to say, that the changes which have taken place in the Museum in that time are more his work than any other man's; and when we find that the number of persons employed has grown from thirteen to over two hundred, and the number of specimens from two hundred thousand to over three million, and consider that what the Museum now is, its scheme and arrangement, with almost all which make it distinctive, are chiefly Dr. Goode's, we have some of the evidence of his administrative capacity. He was fitted to rule and administer both men and things, and the Museum under his management was, as some one has called it, "A house full of ideas and a nursery of living thought."

" His success of administration," says Professor Osborn, " also came partly from an instinctive knowledge of human nature. . . . He sought out the often-latent best qualities of the men around him and developed them. When things went out of joint and did not move his way, he waited with infinite patience for the slow operation of time and common sense to set them right. He was singularly considerate of opinion, . . . fertile of original ideas and suggestions, full of invention and of new expedients, studying the best models at home and abroad, but never bound by any traditions of system or of classification. . . . To all his work also he brought a refined artistic taste, shown in his methods of printing and labeling, as well as in his encouragement of the artistic, and therefore the truthful and realistic development of taxidermy in the arrangement of natural groups of animals. To crown all, like Baird he entered into the larger conception of the wide-reaching responsibilities of his position under the Government, fully realizing that he was not at the head of a university or of a metropolitan museum, but of the Museum of a great Nation. Every reasonable request from another institution met a prompt response. . . . Not the advancement of Washington science, but of American science, was his dominating idea."

There was no subject in connection with the administration of the Museum to which he did not at some time or other give his personal attention. He had a quick eye for color and for form, understood the art of decorating and case-building, and had besides a special knowledge of subjects so widely remote from his own biologic interests that it is a question whether a new species or a new musical instrument gave him the greater pleasure. So fully could I rely on his judgment in all things, that even in matters not connected with the Museum I frequently sought the benefit of his advice, and this was sure to be sound, whether it related to the typography or paper of a new volume of the publications, or to some weighty question of policy. It is difficult to single out from among the manifold matters relating to the Institution proper which were confided to him one single thing. I cannot, however, but recall the fact that he seemed to me, both because of the soundness of his judgment and the wide domain of science with which he was acquainted, the fittest person to place in charge of the Hodgkins award made two years ago. To this entire work, from the time of Mr. Hodgkins' gift, down to the closing of the award, Mr. Goode gave unremitting and zealous attention, having served as chairman both of the preliminary committee and the committee on award.

The field of natural history, of antiquities, of art, of books, is so vast that a mere assemblage of objects, of books, of prints, of engravings, is not in itself significant. Collecting is an art which many essay but few attain. Mr. Goode was eminently a collector. As early as 1872 we find him collecting the fishes of the Bermudas, which he worked up in a catalogue, giving in each case, in addition to characteristics previously noted, descriptions of the colors of the fishes while living, notes on the size and proportions, observations of habits, hints in reference to the origin and meaning of their popular names, and notes upon modes of capture of economic value. The same careful methods of collection he followed in the subsequent expeditions which he undertook in the field. It was not alone in natural history, however, that this talent for collecting displayed itself. Every possible sort of specimen or information which was at hand he collected. He would bring back from every exposition which he attended methodical collections, frequently of materials overlooked by others. Every visit to a foreign country resulted in the bringing back of a collection, not of miscellaneous objects, but of a series which could themselves be placed on exhibition. These might be musical instruments, ecclesiastical art, early printed books, medals, or ivories, and the same taste and discrimination and good judgment were displayed in their selection. He collected, however, not only objects, but also words and ideas. From the assembling of the common names of plants and animals in America, there grew a large collection of Americanisms, probably larger than any single collection published. Portraits of scientific men, portraits of Washington and Jefferson, autographs, Confederate imprints, Americana, American scientific text-books—these are but a few of the fields in which Dr. Goode collected.

He was a naturalist in the broadest sense of that word, following in the footsteps of Agassiz and Baird " He had," says Dr. Gill, " acquaintance with several classes of the animal kingdom, and especially with the vertebrates. He even published several minor contributions on herpetology, the voice of crustacea, and other subjects. . . . The flowering plants also enlisted much of his attention, and his excursions into the fields and woods were enlivened by knowledge of the subjects he met with." " The designation naturalist," says Professor Osborn, " was one which Goode richly earned and which he held most dear, and our deep sorrow is that his activity as naturalist extended only over a quarter of a century." . . . "As a naturalist, Goode did not close any of the windows opening out into Nature. His broad spirit in public affairs displayed itself equally in his methods of field and sea work and in the variety of his observations and writings. While fishes became his chief interest, he knew all the eastern species of birds after identifying and arranging the collection in his college museum. He loved plants, and in the later years of his life took great pleasure in the culture of the old-fashioned garden around his house. . . . Many of his briefer papers deal directly with the biological problems which attracted his interest, especially among reptiles and fishes, touching such questions as migration, coloring, albinism, mimicry, parasitism, feeding and breeding habits, the relation of forest protection to the protection of fishes."

Perhaps no one can be a "naturalist" in the larger sense without being directly a lover of Nature and of all natural sights and sounds. One of his family says, "He taught us all the forest trees, their fruits and flowers in season, and to know them when bare of leaves by their shapes; all the wayside shrubs, and even the flowers of the weeds; all the wild birds and their notes, and the insects. His ideal of an old age was to have a little place of his own in a mild climate, surrounded by his books for rainy days, and friends who cared for plain living and high thinking, with a chance to help some one poorer than he." He was a loving and quick observer, and in these simple, natural joys, his studies were his recreations, and were closely connected with his literary pursuits.

He was of course first and foremost an ichthyologist, and this through no lack of sympathy with the larger field, but because of the recognition of the fact that the larger field could not be successfully covered by one man.

His adherence to this subject as a specialty was undoubtedly determined by his long and intimate connection with the Fish Commission during the period of greatest advancement in methods of deep-sea exploration, the rich collections of fishes derived from that source being placed at his command. The novelties of structure and environment presented by this material, ever increasing as the work progressed, proved an attraction too strong to be resisted, even in the face of his varied official duties, and caused him to become distinctively a student of the marine forms.

His observations were not confined to any single branch of the subject, but were given the widest latitude that his time permitted. He was the discoverer of many new and strange species and an acknowledged authority on classification, but he took perhaps the greatest interest in questions regarding the geographical and bathymetrical distribution of fishes, a field in which his opportunities for investigation had been unexcelled. The color of fishes had also been a favorite study with him, and he had paid attention to many points in their morphology and in the functions of special organs. He was especially well versed in the literature of ichthyology from the earliest times, and, after Professor Baird, was the most eminent exponent in this country of the benefits to be secured to the practical fisheries through the application of scientific teachings.

Dr. Gill in reviewing his scientific career said: "A 'Catalogue of the Fishes of the Bermudas,'[1] published in 1876, furnished additional evidence of knowledge of the literature of his subject and ability to use it to advantage in the discussion of mooted questions, and it also evinced his power of observation.

"In the same year, 1876, appeared another work which to a still greater degree rendered manifest those same mental characteristics. The work was only a catalogue, but perhaps from no other publication can some intellectual qualities be so readily and correctly gauged by a competent judge as an elaborate catalogue. Powers of analysis and synthesis, and the ability to weigh the relative values of the material at hand, may make a 'mere catalogue' a valuable epitome of a collection and of a science. Such a production was the 'Classification of the Collection to illustrate the Animal Resources of the United States,'[†] a work of 126 pages. Three years later this catalogue served as the basis for and was elaborated and expanded into a large 'Catalogue of the Collection to illustrate the Animal Resources and the Fisheries of the United States,'[†] a volume of 351 pages.

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[1] Catalogue of the Fishes of the Bermudas, based chiefly on the collections of the United States National Museum. . . . Washington: Government Printing Office, 1876 (8°, pp. (2) 1–82, Bulletin United States National Museum, No. 5).

[†] International Exhibition, 1876. Board in behalf of United States Executive Departments. Classification of the Collection to illustrate the Animal Resources of the United States: A list of substances derived from the animal kingdom, with synopsis of the useful and injurious animals and a classification of the methods of capture and utilization. . . . Washington: Government Printing Office, 1876 (8°, pp. 126, a Second edition with supplementary title as Bulletin No. 6, United States National Museum).

[‡] International Exhibition, 1876. Catalogue of the Collection to illustrate the Animal Resources and the Fisheries of the United States, exhibited at Philadelphia in 1876 by the Smithsonian Institution and the United States Fish Commission, and forming a part of the United States National Museum. . . . Washington: Government Printing Office, 1879 (8°, pp. 351 (1), Bulletin United States National Museum, No. 14).

These catalogues were for the tentative and adopted arrangement of material exhibited by the Smithsonian Institution and the United States Fish Commission at the 'International Exhibition, 1876.'

"It was the ability that was manifested in these catalogues and the work incidental to their preparation that especially arrested the attention of Professor Baird and marked the author as one well adapted for the direction of a great museum. For signal success in such direction special qualifications are requisite. Only some of them are a mind well trained in analytical as well as synthetic methods, an artistic sense, critical ability, and multifarious knowledge, but above all, the knowledge of men and how to deal with them. Perhaps no one has ever combined in more harmonious proportions such qualifications than G. Brown Goode. In him the National Museum of the United States and the world at large have lost one of the greatest of museum administrators.

"As a naturalist, the attention of Dr. Goode was especially directed to and even concentrated on the fishes. His memoirs, contributed mostly to the Proceedings of the United States National Museum, were numerous and chiefly descriptive of new species. (For many of these he had as a collaborator Dr. Tarleton Bean, then the curator of fishes of the United States National Museum.) Some of the memoirs, however, dealt with special groups, as the Menhaden (1879), Ostraciontidæ (1880), Carangidæ (1881), the Swordfishes (1881), and the Eel (1882). His monograph of the Menhaden (*Brevoortia tyrannus*), contributed originally to the Report of the United States Commissioner of Fisheries * and then published as a separate work †—a large volume of nearly 550 pages and with 30 plates—is a model of critical treatment of information collected from all quarters; but his most important contributions were published as official Government reports and were the results of investigations especially undertaken for such reports. Especially noteworthy were the volumes comprising the results of the Census of 1880.

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*The Natural and Economical History of the American Menhaden. In Report United States Commission of Fish and Fisheries, part V, 1879, Appendix A, pp. 1–529, pls. i–xxxi (xxx canceled), pp. 194–267 by Professor W. O. Atwater.

†American Fisheries: A History of the Menhaden, by G. Brown Goode, with an account of the Agricultural Uses of Fish by W. O. Atwater . . . And an introduction, bringing the subject down to date. Thirty plates. New York: Orange Judd Company, 1880 (8°, pp. x (i), iii–xii, 1–529 (1), 31 pls., pl. 30 canceled).

"The 1880 Census was planned and carried out on an unusual scale. For the fisheries the United States Commission of Fish and Fisheries coöperated, and Dr. Goode had general charge of the entire work. The assistants and special agents were consequently selected with judgment, and the results were very valuable. The huge mass of statistics was digested and condensed in seven large quarto volumes, representing five sections separately devoted to special branches of the subject.*

"Dr. Goode's cares were mainly concentrated on the first section, treating of the 'Natural History of Aquatic Animals,' which was discussed in over 900 pages of text and illustrated by 277 plates. This work was by far the mose complete survey of the economical fishes of the country that had ever appeared, and has since been the most prized; it led to another.

"After the appearance of the census volumes, Dr. Goode was urged to prepare a work for popular use. His consent to do so was followed by a volume, entitled 'American Fishes, a popular treatise upon the game and food fishes of North America,'† published by the Standard Book Company of New York. Inasmuch as none of the previous popular works on the American fishes had emanated from men of scientific eminence, it scarcely need be added that the new work had no rival in the field, so far as accurate information and details of habits were involved.

"A short time previously Dr. Goode had also prepared the text to accompany a series of twenty large folio colored portraits by an eminent artist, Mr. S. A. Kilbourne, of the principal 'Game Fishes of the United States.'*

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* The Fisheries and Fishery Industry of the United States. Prepared through the coöperation of the Commissioner of Fisheries and the Superintendent of the Tenth Census. By George Brown Goode, Assistant Director of the United States National Museum, and a staff of associates. Washington: Government Printing Office, 1884 (–1887, 5 sections in 7 volumes). "Section 1, Natural History of Aquatic Animals," was mainly prepared by Dr. Goode.

†American Fishes: A popular treatise upon the game and food fishes of North America, with especial reference to habits and methods of capture, by G. Brown Goode, with numerous illustrations. New York: Standard Book Company, 1888 (8°, xvi + 496 pp., colored frontispiece).

"Never had investigations of the deep sea been conducted with such assiduity and skill as during the last two decades. The chief honors of the explorations were carried off by the British and American Governments. As the fishes obtained by the vessels of the United States Fish Commission were brought in they were examined by Dr. Goode (generally in company with Dr. Bean) and duly described. At length Drs. Goode and Bean combined together data respecting all the known forms occurring in the abysmal depths of the ocean, and also those of the open sea, and published a résumé of the entire subject in two large volumes entitled 'Oceanic Ichthyology.'†

"This was a fitting crown to the work on which they had been engaged so long, and the actual publication only preceded Dr. Goode's death by about a fortnight.

"But the published volumes did not represent all the work of Dr. Goode on the abyssalian fishes. He had almost completed an elaborate memoir on the distribution of those fishes, and, contrary to the conclusions of former laborers in the same field, had recognized for them a number of different faunal areas. It is to be hoped that this may yet be given to the world.

"Morphological and descriptive ichthyology were not cultivated to the exclusion of what is regarded as more practical features. In connection with his official duties as an officer of the United States Fish Commission he studied the subject of pisciculture in all its details. Among his many contributions to the subject are one on 'The First Decade of the United States Commission, its plan of work and accomplished results, scientific and economical' (1880), another treating of the 'Epochs in the History of Fish Culture' (1881), and two encyclopædic articles 'The Fisheries of the World' (1882), and the one entitled 'Pisciculture,' in the Encyclopædia Britannica (1885)."

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*Game Fishes of the United States, by S. A. Kilbourne; text by G. Brown Goode. New York: Published by Charles Scribner's Sons, 1879–1881. (Folio, 46 pp., 20 plates, and map, published in ten parts, each with 2 plates, lithographs in water color, and four-page folio of text.)

†Smithsonian Institution. United States National Museum. Special bulletin. Oceanic Ichthyology: A Treatise on the Deep-sea and Pelagic Fishes of the World, based chiefly upon the collections made by the steamers *Blake*, *Albatross*, and *Fish Hawk* in the Northwestern Atlantic, with an atlas containing 417 figures, by George Brown Goode, Ph. D., LL. D., and Tarleton H. Bean, M. D., M. S. Washington: Government Printing Office, 1895, 2 vols., 4°; I, xxxv + 26, 553 pp.; II, xxiii + 26 pp., 123 pl.

"The great work of his life, 'Oceanic Ichthyology,'" says Dr. Jordan, "was, however, written during the period of his directorship of the National Museum, and it was published but a month before his death. Almost simultaneously with this were other important publications of the National Museum, which were his also in a sense, for they would never have been undertaken except for his urgent wish and encouragement. If a personal word may be pardoned, 'The Fishes of North and Middle America,' which closely followed 'Oceanic Ichthyology,' would never have been written except for my friend's repeated insistance and generous help.

"The first recorded scientific paper of Dr. Goode is a note* on the occurrence of the bill-fish in fresh water in the Connecticut river. The next is a critical discussion of the answers to the question, 'Do snakes swallow their young?' In this paper he shows that there is good reason to believe that in certain viviparous snakes the young seek refuge in the stomach of the mother when frightened, and that they come out unharmed when the reason for their retreat has passed.

"The first of the many technical and descriptive papers on fishes was the 'Catalogue of the Fishes of the Bermudas,'† published in 1876. This is a model record of field observations and is one of the best of local catalogues. Dr. Goode retained his interest in this outpost of the great West Indian fauna, and from time to time recorded the various additions made to his first Bermudan catalogue.

"After this followed a long large number of papers on fishes, chiefly descriptions of species or monographs of groups. The descriptive papers were nearly all written in association with his excellent friend, Dr. Tarleton H. Bean, then curator of fishes in the National Museum.

"In monographic work Dr. Goode took the deepest interest, and he delighted especially in the collection of historic data concerning groups of species. The quaint or poetical features of such work were never overlooked by him. Notable among these monographs are those of the Menhaden, the Trunk-fishes, and the Sword-fishes.

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* American Naturalist, vol. v, p. 487.

† Bulletin No. 5, United States National Museum.

" The economic side of science also interested him more and more. That scientific knowledge could add to human wealth or comfort was no reproach to him. In his notable monograph of the Menhaden* the economic value as food or manure of this plebeian fish received the careful attention which he had given to the problems of pure science.

" Dr. Goode's power in organizing and coördinating practical investigations was shown in his monumental work† on the American Fisheries for the Tenth Census, 1880. The work of the record of the fisheries and associated marine industries was placed in his hands by Francis A. Walker, Superintendent of the Census. Under Dr. Goode's direction skilled investigators were sent to every part of the coast and inland waters of the country."

His "American Fishes," a popular treatise upon the game and food fishes of North America, published in 1888, is deserving of a special mention both because of the charming literary style in which it is written as well as its scientific accuracy and excellence. The wealth and aptness of the chapter headings of this book show that Mr. Goode's wide reading was associated with everything which could illustrate his science on the literary side. He had a knowledge of everything even remotely connected with his ichthyological researches from St. Anthony's " Sermon to Fishes " to the literature of fish cookery, while in one of his earliest papers, written at nineteen, his fondness for Isaac Walton and his familiarity with him are evident.

While never claiming the title of anthropologist, he was yet a close student of the anthropological and ethnological work in this country and abroad, and it is not too much to say that no professional anthropologist had a higher ideal of what his science might come to be or exercised a more discriminating criticism on its present methods and conditions than did Dr. Goode. He was, moreover, not only interested in the biological problems of the anthropologist, but in technology and the history of art; the history of human invention and archæology were equally in his mind, and his suggestiveness in each of these fields could be attested by all of the anthropologists with whom he came in contact.

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* "The Natural and Economical History of the American Menhaden." In Report of United States Commission of Fish and Fisheries, part 5. Washington, 1879.

† "The Fisheries and Fishery Industry of the United States." Prepared through the coöperation of the Commissioner of Fisheries and the Superintendent of the Tenth Census. Washington, 1884.

"It would be difficult," says Professor Mason, "to find among those who are professional anthropologists a man who had a more exalted idea of what this science ought to be. There is not perhaps another distinguished scholar who has endeavored to collect into one great anthropological scheme all of the knowledge of all men in all ages of the world and in all stages of culture."

Dr. Goode was peculiarly related to the management of expositions and did more than any other person in America to engraft upon them museum ideas and widen their scope from the merely commercial and industrial to the educational and scientific.

His first experience in this field was in 1876, at the Centennial Exhibition held in Philadelphia. Professor Baird was in charge of the exhibits of the Smithsonian Institution and Fish Commission, and being much occupied at the time with other matters, the greater part of the installation and other work connected with the exhibit was placed under the immediate supervision of Mr. Goode. The work done by the Smithsonian and Government Departments at this exhibition was pioneer work, it being the first international exhibition in which the United States Government was engaged. It is not too much to say that the arrangement of the Smithsonian exhibit at Philadelphia was the model on which all subsequent exhibits of the kind were based, and that the classification, the installation, and the arrangement have had a lasting influence on exhibition work everywhere. But every administrative activity of this sort was sure to result in some literary product, so that we find in 1876 Mr. Goode published "A Classification of the Collections to illustrate the Animal Resources of the United States: A list of substances derived from the animal kingdom, with synopsis of the useful and injurious animals, and a classification of the methods of capture and utilization." This work was afterwards published in an enlarged form as a bulletin of the National Museum.

His services as commissioner for the United States Government at the Fisheries Exhibition of Berlin in 1880 and London in 1883 have already been alluded to. These, too, resulted in several articles in German and in a bulletin of the Museum, while several addresses and papers delivered at the Conferences of the International Fisheries Exhibition in London were published in the papers of the conferences, and full reports were made by Dr. Goode on his return to this country and published at the Government Printing Office.

He was the representative of the Smithsonian Institution at all the subsequent exhibitions held in this country—Louisville, 1884; New Orleans, 1885; Cincinnati, 1888; Chicago, 1893, and Atlanta, 1895—serving also as a commissioner and for a time acting Commissioner General to the Columbian Exposition held at Madrid in 1892.

The exhibits made under his direction were never repetitions. Each one contained new material never shown before, and exhibited the progress of the Institution and Museum, as well as the advances made in the arts of taxidermy, installation, and labelling. Mr. Goode, too, always bore in mind the local interest, and endeavored to show specimens and materials which would be instructive to persons residing in the neighborhood of the place at which the exposition was held. Thus at Cincinnati objects were prominent which related to the Ohio valley, for Madrid he prepared an exhibit to illustrate the conditions of human and animal life in America at the time of the Spanish discovery, whilst at Atlanta especial stress was laid on showing the fauna, flora, archæology, and mineral resources of the south Atlantic states. He prepared the report on the Madrid Exposition, and at the request of the Government Commission drew up a provisional classification for the Chicago Exposition, which, while not formally accepted, was used throughout in the official classification, many pages being copied without a change. For the Chicago, as well as the Atlanta Exposition, he prepared a carefully written catalogue, and for the latter an excellently condensed sketch of the Smithsonian Institution.

Nowhere were Mr. Goode's administrative talents more strongly shown than in an exhibition. The plans of the floor space, the cases, the specimens were all carefully arranged in advance. Boxes were especially made of lumber which could be utilized for cases or platforms. Cases were marked, and not very long before the opening of the exposition the entire mass would be deposited on the bare space assigned to the Smithsonian exhibit. Usually other exhibitors had their material half arranged by this time, and the fear was expressed by sympathetic bystanders that the Smithsonian would not be ready. The cases would be unpacked and the specimens put in them in whatever position they happened to stand, and up to the last day all would seem to be in confusion; but Dr. Goode knew his resources and his men as a general knows his army. Suddenly all detailed work would come to an end, and in the course of a few hours, as if by magic, the entire exhibit would be put in place. He had a pardonable pride in this sort of generalship, for whether at Chicago or Atlanta it had never failed him, and it earned the highest encomiums at Berlin, London, and Madrid.

Dr. Goode's services at these various expositions were recognized by diplomas and medals, and from the Spanish government he received the order of Isabella the Catholic, with the grade of commander.

I have already spoken of Mr. Goode's administrative qualities as shown in his management of the National Museum; but his contributions to museum administration and the history of museums were not confined to his own work. From all parts of America and even as far distant as Australia his opinion was sought with regard to the plans for museum buildings as well as on minor matters of installation. All requests for such information and advice were fully answered in minute detail.

It was into his papers on museums that some of his best thoughts went, and it was there that we find epigrammatic statements which are constantly quoted by all interested in the matter.

The first paper by him on this subject appeared in the "College Argus" March 22, 1871. It was entitled "Our Museum," and was a description of the collection in Judd Hall. This article indicated plainly the museum instinct, for it was largely intended to make known the deficiencies in the collection, and pointed out how students and professors could make these good on their summer excursions. He also published a guide to this museum.

In 1888 he read before the American Historical Association a paper entitled "Museum History and Museums of History."

Here he traced the growth of the museum idea from the beginning down to the present time, repeating his now oft-quoted phrase, "An efficient educational museum may be described as a collection of instructive labels, each illustrated by a well selected specimen." Atlases of ethnological portraits and works like those of Audubon he described as "not books, but museum specimens masquerading in the dress of books."

Even more forcible was a lecture delivered before the Brooklyn Institute in 1889, entitled "Museums of the Future." "The museum of the past," he wrote, "must be set aside, reconstructed, transformed from a cemetery of bric-a-brac into a nursery of living thoughts." . . . "The people's museum should be much more than a house full of specimens in glass cases. It should be a house full of ideas, arranged with the strictest attention to system." . . . "A finished museum is a dead museum, and a dead museum is a useless museum."

Most noteworthy, however, was his paper contributed to the Museums' Association of Great Britain in 1895, entitled "The Principles of Museum Administration." This was a carefully prepared codification of "the accepted principles of museum administration," which Mr. Goode hoped would "be the cause of much critical discussion." The ideas were presented in the form of aphorisms and were exceptionally clear cut, ending with the assertion that "the degree of civilization to which any nation, city, or province has attained is best shown by the character of its public museums and the liberality with which they are maintained."

This paper was warmly welcomed by museum experts, many of whom testified by their letters the interest they had in the clear presentation of the principles which should guide the museum administrator. At the 1896 meeting of the same Association Mr. Bather said: "When I read the magnificently exhaustive address by Dr. G. Brown Goode, published in our last report, it was manifest that all the ideas I had ever had were anticipated in that masterly production;" whilst an obituary note in the same volume says, "His early death is a great loss, not only to the United States National Museum, but museums in general, for he took a deep and active interest in all things affecting their development and well-being."

The "Manchester Guardian," September 20, 1896, says: "He was a recognized authority on all matters affecting museum administration, and in this capacity he last year wrote a paper on the principles of museum management and economy, which was brought before the annual Congress of the Museums Association at Newcastle, and has since attracted much attention as an admirable exposition of the general theory of administration applicable to museum work in all its branches. It is of interest to note that Dr. Goode's definition of a museum is an institution for the preservation of those objects which best illustrate the phenomena of nature and the works of man, and the utilization of these for the increase of knowledge and for the culture and enlightenment of the people. In this spirit Dr. Goode worked, and he not only achieved much in his own country, but was also ever ready to cordially coöperate with foreign kindred institutions, especially those in England, for the advancement of museum work as a means of education."

These activities would have been sufficient for an ordinary man, but in addition he was the historian of American science.

In 1886 he delivered, as president of the Biological Society of Washington, an address entitled "The Beginnings of Natural History in America," tracing it from Thomas Harriott, who came to this country in 1585, reciting the scientific labors of Captain John Smith, John Ray, Thomas Jefferson, and a host of others. The spirit which actuated this address is well illustrated in the following paragraphs: "It seems to me unfortunate, therefore, that we should allow the value of the labors of our predecessors to be depreciated, or to refer to the naturalists of the last century as belonging to the unscientific or the archaic period. It has been frequently said by naturalists that there was no science in America until after the beginning of the present century. This is, in one sense, true; in another, very false. There were then, it is certain, many men equal in capacity, in culture, in enthusiasm to the naturalists of today, who were giving careful attention to the study of precisely the same phenomena of nature. The misfortune of the men of science in 1785 was that they had three generations fewer of scientific predecessors than we."

This address he followed up by a second, entitled "The Beginnings of American Science: The Third Century," delivered in 1888, also before the Biological Society. He divided the period from 1782 to 1888 into three periods, which he called after the names of Jefferson, Silliman, and Agassiz.

Continuing along this same line, he contributed to the American Historical Association, in 1890, a paper on "The Origin of the National Scientific and Educational Institutions of the United States."

The material contained in these various papers was summed up in an unpublished work entitled "What has been done for Science in America, 1492–1892," which illustrates in an interesting way the development of Dr. Goode's mind, for in this study as much attention is given to astronomy, physics, and even comparative philology as is paid to natural history. Parallel with this work may be mentioned a collection of portraits of almost every scientific man of importance mentioned in any of these four essays. Besides these, he wrote an article in the "Science News," 1878, entitled "The Earliest American Naturalist, Thomas Harriott."

He was greatly interested in American history, a close student of the writings of the fathers—more especially of Washington and Jefferson—and an enthusiastic investigator of Virginia history, for which he had assembled a great mass of original material. He was especially interested in the study of institutional history, which he thought approximated most nearly to the scientific method. It is more than likely that this interest grew out of his studies in genealogy, the most splendid result of which is his "Virginia Cousins," though a great mass of material, still unpublished, attests the fact that these genealogical collections were intended to cover the South and to serve as a contribution to southern history. He relates in the prologue to his "Virginia Cousins" that his interest in the Goode family tree was awakened in him by his father at the age of twelve.

The significance of genealogical studies for American history he recognizes in the following words: "The time is coming when the sociologist and the historian will make an extensive use of the facts so laboriously gathered and systematically classified by genealogists, and it is probable that this can be better done in the United States than elsewhere;" and again, "One of the elements of satisfaction in genealogical study legitimately arises from the success of our attempts to establish personal relations with past ages and to be able to people our minds with the images of our forefathers as they lived two, three, four hundred years ago."

But there was a scientific interest which attached to this work, as well as an historical one, for Dr. Goode was a strong believer in heredity, and he was profoundly impressed with the idea that man's capabilities and tendencies were to be explained by the characteristics of the men and women whose blood flowed through their veins.

This idea, too, is brought out strongly in his biographical work, nowhere more strongly than in his biographies of Henry, Baird, and Langley (almost the last work he ever did) for the Smithsonian Half Century Book, and it is carefully worked out in an elaborate plan of a biography of Professor Baird, which would probably have been the next literary work he would have undertaken had his life been spared.

He was greatly interested in bibliography, his methods in this science being most exact. He published bibliographies of Spencer Fullerton Baird, Charles Girard, Philip Lutley Sclater, and had under way bibliographies of Theodore Gill and David Starr Jordan. "A gigantic work in the same line," says Dr. Gill, "had been projected by him and most of the materials were collected; it was no less than a complete bibliography of ichthyology, including the names of all the genera and species published as new. In no way may ichthyology at least more feel the loss of Goode than in the loss of the complete bibliography."

Mr. Goode was a student of the history of the scientific societies, and was himself deeply interested in their welfare. In all the Washington scientific societies he was an active member, serving as president both of the Biological Society and the Philosophical Society, before which he delivered his notable address on the history of American science. He also belonged to the Anthropological and Geographic Societies of Washington and stoutly maintained the traditions of all these. He was elected a member of the National Academy of Sciences in 1888, was for many years a member of the Association for the Advancement of Science, being elected vice-president of the Zoölogical Section last summer, a few days before his death. He was a member of the American Philosophical Society, of the American Society of Naturalists, and a Fellow of the American Academy of Arts and

Sciences, and among foreign societies he had been honored by election to the Société des Amis des Sciences Naturelles de Moscou, Société Zoölogique de France, Zoölogical Society of London, and the Société Scientifique du Chile.

He seemed to regard historical and patriotic societies with an equal interest, being a member of the council of the American Historical Association and a member of the Virginia Historical Society and the Columbian Historical Society of Washington and of the newly formed Southern Historical Society. His work in connection with the hereditary and patriotic societies was so especially near to him as to demand an unusual mention. He was one of the organizers of the Sons of the American Revolution of the District of Columbia, holding the offices of vice-president general and registrar general in the national society, and at the time of his death of president in the local society. He stimulated this society to issue historical publications, and saw a number through the press himself. A society known as the Sons of the Revolution having been founded with somewhat similar aims, Mr. Goode joined this organization with the avowed purpose of bringing them together. In this society he held the office of vice-president. He was lieutenant governor of the Society of Colonial Wars of the District of Columbia. He gave constant advice to the Daughters of the American Revolution during the period of their organization, and was instrumental in having the State of Massachusetts present, as a home for the Daughters of the Revolution in Georgia, its building at the Atlanta Exposition, which was a copy of the old Craigie house in Cambridge, once occupied by Washington as his headquarters, and later the residence of Longfellow. The success of this effort gave him special pleasure, for he regarded it as one of the means for promoting friendliness between the people of New England and the people of the South.

Although these numerous duties and activities would seem to have been more than enough for any single man, Mr. Goode did not stop here. Every scientific activity of the Government had at some time or other the advantage of his wise counsel and his active coöperation. His public duties outside of the Smithsonian in connection with the Department of State, the Fish Commission, the Census, and the various expositions abroad at which he represented this Government I have already alluded to; but he was possessed of a higher order of patriotism which even this service did not satisfy. Mr. William L. Wilson, Regent of the Smithsonian Institution, lately Postmaster General of the United States and president of Washington and Lee University, says: "He was a richly endowed man—first, with that capacity and that resistless bent toward the work in which he attained his great distinction that made it a perennial delight to him; but he was scarcely less richly endowed in his more unpretending and large human sympathies, and it was this latter endowment that distinguished him as a citizen and as a historian.

"As a citizen he was full of patriotic, American enthusiasm. He understood, as all must understand who look with seriousness upon the great problems that confront a free people and who measure the difficulties of those problems—he understood that at least one preparation for the discharge of the duties of American citizenship was the general education of the people, and so he advocated, as far as possible, bringing down to the reach of all the people, not only the opportunities, but the attractions and the incitements to intellectual living.

"Dr. Goode, with the quick and warm sympathies of the man and of the historian, seems to have felt that he could do no greater service to the people of his day and generation and to his country than in the most attractive and concrete way, if I may so express it, to lead the young men of this country to a study of the history of the past—to the deeds and the writings of the great men to whom we owe the foundation and the perpetuation of our institutions."

He was greatly interested in the establishment of a National University, and in 1891 read a paper in Philadelphia, afterwards printed in the magazine "Lend a Hand," edited by Edward Everett Hale, entitled "Washington's University the Nation's Debt of Honor." In this article he computed that the bequest of Washington to the United States for a National University would, at compound interest, amount, in 1892, to four million one hundred thousand dollars, and he proposed that the National Government should restore this sum as the nucleus of the endowment for the National University. He acted as secretary of the executive committee, of which the Chief Justice was chairman, which was laboring to this end, and spared no effort to bring it to a successful conclusion.

Another project in which he was interested and for which he labored was a movement to fully open French universities to American students. His interest was excited in this movement because he thought that American science was becoming one-sided, owing to the fact that all of the students who went abroad visited German universities. Of the American committee, which, in coöperation with the French committee, had this matter in charge, Dr. Goode was the secretary, and he had the satisfaction of seeing this project brought to a successful issue before his death.

He had a strong interest in literature, and wrote in an excellent English style—clear, direct, and unpretentious. I have never met a mind in touch with more far-away and disconnected points than his, nor one of the same breadth and variety of writing, outside of the range of his own specialty. He had fine æsthetic tastes, and derived keen enjoyment from everything that was beautiful in nature or in art. He knew all natural sights and sounds, and recognized the note of every bird. He knew good pictures and good prints, was familiar with all the processes of graphic arts, and a good judge of them, both on the technical and the artistic side. He loved a beautifully printed book and an artistic binding. All these tastes he utilized in the publications which he wrote or edited. The work which he had in hand at the time of his death and to which he devoted so much loving care, the History of the First Half Century of the Smithsonian Institution, he aimed to make the expression of all these tastes. To no writing which he ever did, did he bring a higher literary expression than to the pages which he prepared for this book. He was at infinite trouble in discussing such matters as the form of the page, the style of the type, the quality of the paper, the initial letters, the head lines and illustrations, and the binding, and when he discussed any of these points with the expert craftsmen his knowledge of the details was as full as their own.

In spite of ill health and suffering, his overwrought, nervous system and his occasional severe mental depression, he never allowed himself to take a cynical view of human nature. He was a man who loved his fellow-men, and to whom that love was repaid with a warmth to a degree rare in this day. He made all other men's concerns his own. He sent notes and suggestions to hundreds of scientific men, whose work profited thereby, and in the large circle of friends he had, scarcely one did not at one time or other come to Mr. Goode for advice and sympathy upon his own private affairs. He was an intensely loyal American patriot, ever careful that nothing should be said or done that should in any way reflect upon his country. He was especially devoted to Virginia and never happier than when he could spend a few days on her soil, looking over a historic house or copying some of the records which he hoped to turn to advantage in his historical studies.

"He is remembered," says Dr. Dall, "as one never weary of well-doing; who reached the heights, though ever aiming higher; whose example stimulated and whose history will prove a lasting inspiration."

"As a public-spirited naturalist," says Professor Osborn, "he leaves us a tender memory and a noble example, which helps us and will help many coming men into the higher conception of duty in the service and promotion of the truth. We cannot forget his smile nor his arm passing through the arm of his friend."

I have never known a more perfectly sincere and loyal character than Dr. Goode's, or a man who, with better judgment of other men or greater ability in molding their purposes to his own, used these powers to such uniformly disinterested ends, so that he could maintain the discipline of a great establishment like the National Museum while still retaining the personal affection of every subordinate.

I have scarcely alluded to his family life, for of his home we are not to speak here, further than to say that he was eminently a domestic man, finding the highest joys that life brought him with his family and children.

He has gone; and on the road where we are all going there has not preceded us a man who lived more for others, a truer man, a more loyal friend.

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THE NEW SPECTRUM.

BY

S. P. LANGLEY.

FROM THE SMITHSONIAN REPORT FOR 1900, PAGES 683–692
(WITH COLORED PLATE).

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1901.

THE NEW SPECTRUM.

By S. P. LANGLEY.[1]

The writer (at the concluding meeting of the National Academy of Sciences on April 18) remarked on the disadvantages in the matter of interest of the work of the physicist, which he was about to show them, to that of the biologist, which was concerned with the ever absorbing problem of life. He had, however, something which seemed to him of interest, even in this respect, to speak of, for it included some indications he believed to be new, pointing the way to future knowledge of the connection of terrestrial life with that physical creator of all life, the sun.

He had to present to the academy a book embodying the labor of twenty years, though at this late hour he could scarcely more than show the volume with a mention of the leading captions of its subject. What he had to say then would be understood as only a sort of introductory description of the contents of the work in question, which was entitled "Volume I of the Annals of the Astrophysical Observatory of the Smithsonian Institution."

In illustration of a principal feature of this book, the academy saw before them on the wall an extended solar spectrum, only a small portion of the beginning of which, on the left, was the visible spectrum known to Sir Isaac Newton. This was the familiar visible colored spectrum which we all have seen and know something of, even if our special studies are in other fields.

It is chiefly this visible part, which has been hitherto the seat of prolonged spectroscopic investigation, from a little beyond the violet, at a wave-length of somewhat less than 0.4^{μ} down to the extreme red, which is generally considered to terminate at the almost invisible line A, whose wave-length is 0.76^{μ}. On the scale of the actual wave-length of light, then, where the unit of measurement (1^{μ}) is one one-thousandth of a millimeter, the length of the visible spectrum is 0.36^{μ}.

The undue importance which this visible region has assumed, not only in the eyes of the public, but in the work of the spectroscopist, is easily intelligible, being due primarily to the evident fact that we all possess, as a gift from nature, a wonderful instrument for noting the sun's energy in this part, and in this part only.

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[1] Abstract of a paper read before the National Academy of Sciences at its Washington meeting April 18, 1901.

While, then, this part alone can be *seen* by all, yet the idea of its undue importance is also owing to the circumstance that the operation of the ordinary prism gives an immensely extended linear depiction of the really small amount of energy in this visible part. There is also a region beyond the violet, most insignificant in energy and invisible to the eye, and the association of this linear extension due to the prism, with the accident that the salts of silver used in photography are extraordinarily sensitive to these short wave-length rays, so that they can depict them even through the most extreme enfeeblement of the energy involved in producing them, also makes this part have undue prominence. This action of the prism and of the photograph is local, then, and peculiar to the short wave-lengths; and owing to it, all but special students of the subject are, as a rule, under a wholly erroneous impression of the relative importance of what is visible and what is not. The spectrum has really no positive dimension, being extended at one end or the other according to the use of the prism or grating employed in producing it. Perhaps the only fair measurement for displaying a linear representation of the energy would be that of a special scheme, which the writer had proposed, in which the energy is everywhere the same;[1] but this presentation is unusual and would not be generally intelligible without explanation.

The map before us will be intelligible when it is stated that it is, as to the infra-red, an exact representation of that part of the spectrum given by a rock-salt prism. The visible and ultra-violet spectrum given here is not exact, for the reason that it would take nearly a hundred *feet* of map to depict it on the prismatic scale, though this is caused by but a small fraction of the sun's energy; so monstrous is the exaggeration due to the dispersion of the prism.

Looking, then, at the map: First, in the spectrum on the left and beyond 0.4^{μ} is the ultra-violet region, in fact almost invisibly small, but which in most photographs shows almost a *hundred times larger than the whole infra-red.* It really contains much less than one-hundredth part of the total solar energy which exists. Beyond it is the visible spectrum, containing perhaps one-fifth the solar energy.

As the writer has elsewhere said, "the amount of energy in any region of the spectrum, such as that in any color, or between any two specified limits, is a definite quantity, fixed by facts, which are independent of our choice, such as the nature of the radiant body or the absorption which the ray has undergone. Beyond this Nature has no law which must govern us."

Everything in the linear presentation, then, depends on the scale adopted. In other words, if we have the lengths proportionable to the energies, the familiar prismatic representation enormously exaggerates the importance of the visible, and still more of the ultra-violet region, and similarly the grating spectrum exaggerates that of the infra-red region. Now he had given, on the map before them, and through the whole infra-red, the exact rock salt prismatic spectrum, but for the purpose of obtaining a length which represented (though insufficiently) that of the visible spectrum, he had laid the latter down on the *average* dispersion in the infra-red, which was perhaps as fair a plan as could be taken for showing the approximate relation of the two fields of energy in an intelligible way, though it gave the visible energy too small.

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[1] American Journal of Science, III, xxvii, p. 169, 1884.

Let us recall, then, at the risk of iteration, that in spite of the familiar extended photographic spectra of the hundreds of lines shown in the ultra-violet, and in those of the colored spectrum, it is not here that the real creative energy of the sun is to be studied, but elsewhere, on the right of the drawing, in the infra-red. Looking to the spectrum as thus delineated, next to the invisibly small and weak ultra-violet, comes the visible or Newtonian spectrum, which is here somewhat insufficiently shown, and on the right extends the great invisible spectrum in which four-fifths of the solar energies are now known to exist.

Of this immense invisible region nothing was known until the year 1800,[1] when Sir William Herschel found heat there with the thermometer.

After that little was done[2] (except an ingenious experiment by Sir John Herschel[3] to show that the heat was not continuous) till the first drawing of the energy curve by Lamansky,[4] in 1871, which, on account of its great importance in the history of the subject, is given on the map. It consists of the energy curves of the visible spectrum, and beyond it, on the right (and in illustration of what has just been said it will be seen how relatively small these latter appear), of three depressions indicating lapses of heat in the infra-red. It is almost impossible to tell what these lapses are meant for, without a scale of some kind (which he does not furnish), but they probably indicate something, going down to near a wave-length of 1^{μ}. It is obvious that the detail is of the very crudest, and yet this drawing of Lamansky's was remarkable as the first drawing of the energy spectrum. It attracted general attention, and was the immediate cause of the writer's taking up his researches in this direction.

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[1] Philosophical Transactions, vol. xc, p. 284, 1800.

[2] It should, however, be mentioned that an important paper by Draper (London, Ed. Dublin Phil. Mag., May, 1843) was published in 1843, in which he appears to claim the discovery of the group here called $\rho\sigma\tau$ and which is now known to have a wave-length of less than 1μ. (Its true wave-length was not determined till much later.) Later, Fizeau seems to have found further irregularities of this heat as long ago as 1847, and of its location, obtaining his wave-lengths by means of interference bands. His instrumental processes, though correct in theory, were not exact in practice; and yet it seems pretty clear that he obtained some sort of recognition of a something indicating heat, as far down as the great region immediately above Ω on our present charts. Mouton (Comptes Rendus, 1879) confirmed this observation of Fizeau's and contrived to get at least an approximate wave-length of the point where the spectrum (to him) ended, at about 1.8μ.

[3] Philosophical Transactions, vol. cxxx, p. 1, 1840.

[4] Monatsberichte der k. Akademie der Wissenschaften zu Berlin, December, 1871.

It seems proper to state here that the true wave-lengths were at that time most imperfectly known, but that in 1884, and later in 1885,[1] they were completely determined by the writer as far as the end of what he has called "the new spectrum" at a wave-length of 5.3^{μ}.

The upper portion of the infra-red is quite accessible to photography, and the next important publication in this direction was that of Captain (now Sir William) Abney,[2] which gave the photographic spectrum down to about 1.1^{μ}, much beyond which photography has never mapped since.

From the time of seeing Lamansky's drawing, the writer had grown interested in this work, but found the thermopile, the instrument of his predecessors, and the most delicate then known to science, insufficient in the feeble heat of the grating spectrum, and about 1880 he had invented the bolometer[3] and was using it in that year for these researches. This may perhaps seem the place to speak of this instrument, though with the later developments which have made it what it is to-day, it has grown to something very different from what it was then.

It has, in fact, since found very general acceptance among physicists, especially since it has lately reached a degree of accuracy, as well as of delicacy, which would have appeared impossible to the inventor himself in its early days.

It may be considered in several relations, but notably as to three: (1) Its sensitiveness to small amounts of heat; (2) the accuracy of measurement of those small amounts; and (3) the accuracy of its measurements of the position of the source of heat.

As to the first, it is well known that the principle of the instrument depends on the forming of a Wheatstone bridge, by the means of two strips of platinum or other metal, of narrow width and still more limited thickness, one of which only is exposed to the radiation. In some bolometers in use, for instance, the strip is a tenth of a millimeter, or one two-hundred-and-fiftieth of an inch in width; and yet it is to be described as only a kind of tape, since its thickness is less than a tenth of this.

The use of the instrument is then based on the well-known fact that the heating of an ordinary metallic conductor increases its resistance, and this law is found to hold good in quantities so small that they approach the physically infinitesimal. In the actual bolometers, for instance, the two arms of a Wheatstone bridge are formed of two strips of platinum, side by side, one of which is exposed to the heat and the other sheltered. The warming of the exposed one increases its resistance and causes a deflection of the galvanometer.

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[1] American Journal of Science, March, 1884, and August, 1886.

[2] Philosophical Transactions, vol. clxxi, p. 653, 1880.

[3] Actinic balance, American Journal of Science, 3d series, vol. xxi, p. 187, 1881.

It was considered to be remarkable twenty years ago that a change of temperature of one ten-thousandth of a degree Centigrade could be registered; it is believed at present that with the consecutive improvements of the original instrument and others, including those which Mr. Abbot, of the Smithsonian Institution Observatory, has lately introduced into its attendant galvanometer, less than one *one-hundred-millionth* of a degree in the change of temperature of the strip can be registered. This indicates the sensitiveness of the instrument to heat.

As to the second relation, some measures have been made on the steadiest light source obtainable. With ordinary photometric measures of its intensity one might expect a probable error of about 1 per cent. The error with the bolometer was insensible by any means that could be applied to test it. It is at any rate less than two one-hundredths of 1 per cent. If we imagine an absolutely invisible spectrum, in which there nevertheless are interruptions of energy similar to those which the eye shows us in the visible, then the bolometer, whose sensitive strip passes over a dark line in the spectrum, visible or invisible (since what is darkness to the eye is cold to it), gives a deflection on the side of cold, and in the warmer interval between two lines a deflection on the side of heat; these deflections being proportionate to the cause, within the degree of accuracy just stated.

The third quality, the accuracy of its measures of position, is better seen by a comparison and a statement, for if we look back to the indications of the lower part of Lamansky's drawing we may see that at least a considerable fraction of a degree of error must exist there in such a vague delineation. Now, in contrast with this early record, the bolometer has been brought to grope in the dark and to thus feel the presence of narrow Fraunhofer-like lines by their cooler temperature alone, with an error of the order of that in refined astronomical measurement; that is to say, the probable error, in a mean of six observations of the relative position of one of these invisible lines, is less than one second of arc; a statement which the astronomer, perhaps, who knows what an illusive thing a second of arc is, can best appreciate.

The results of the writer's labors with the bolometer in the years 1880 and 1881, and in part of his expedition in the latter year to Mount Whitney, were given at the Southampton meeting of the British Association for the Advancement of Science in 1882.[1] During these two years very many thousand galvanometer readings were taken, by a most tryingly slow process, to give the twenty or more interruptions shown at that time, below the limit of 1.1^{μ} of Abney's photographs. The bolometer has been called an eye which sees in the dark, but at that time the "eye" was not fairly open, and having then not been brought to its present rapidity of use, the early results were attained only by such unlimited repetition, and almost infinite patience was needed till what was inaccurate was eliminated.

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[1] Report British Association, 1882. Nature xxvi, 1882.

Several hundreds at least of galvanometer readings were then taken to establish the place of *each* of the above twenty lines during the two years when they were being hunted for, and this patience so far found its reward that they have never required any material alteration since, but only additions such as the writer can now give. The part below 1.1^{μ} he then presented (at the Southampton meeting of the British association) as having been mapped for the first time. Mouton had two years before obtained crude indications of heat as far as 1.8^{μ}, and Abney had, as stated, obtained relatively complete photographs of the upper infra-red extending to about this point (1.1^{μ}).

The writer had already determined for the first time by the bolometer, at Allegheny and on Mount Whitney, the wave-lengths of some much remoter regions, including, in part, the region then first discovered by him and here called "the new spectrum," and was able to state that the terminal ray of the solar spectrum, whose presence had *then* been certainly felt by the bolometer, had a wave-length of about 2.8^{μ}, or nearly two octaves below the "great A" of Fraunhofer.

He stated in this communication of 1882 that the galvanometer then responded readily to changes of temperature in the bolometer strip, of much less than one ten-thousandth of a degree Centigrade, (as has just been said, it now responds to changes of less than one one-hundred-millionth), and he added: "Since it is one and the same solar energy, whose manifestations are called 'light' or 'heat' according to the medium which interprets them, what is 'light' to the eye is 'heat' to the bolometer, and what is seen as a dark line by the eye is felt as a cold line by the sentient instrument. Accordingly, if lines analogous to the dark 'Fraunhofer' lines exist in this invisible region, they will appear (if I may so speak) to the bolometer as cold bands, and this hair-like strip of platinum is moved along in the invisible part of the spectrum till the galvanometer indicates the all but infinitesimal change of temperature caused by its contact with such a 'cold band.' The whole work, it will be seen, is necessarily very slow; it is, in fact, a long groping in the dark and it demands extreme patience."

At that time it may be said to have been shown that these interruptions were due to the existence of something like dark lines or bands, resembling what are known as the Fraunhofer lines in the upper spectrum; but, apart from what the writer had done, no one then surmised how far this spectrum extended nor, perhaps, what these explorations really meant. They may be compared to actual journeys into this dark continent, if it may be so called, which extended so far beyond those of previous explorers that the determination of positions by the writer, corresponding somewhat to longitudes determined by the terrestrial explorer in a new country, was, by those who had not been so far but had conceived an inadequate idea of the extent of the region, treated as erroneous and impossible.

A necessary limit to the farthest infra-red was in 1880 supposed to exist near the wave-length 1^μ. Doctor John Draper,[1] for instance, announced in other terms that the extreme end of the invisible spectrum might, from theoretical considerations, be probably estimated at something less than the wave-length of 1^μ, whence it followed that the above value of 1.8^μ was impossible, and, still more, that of 2.8^μ. If, in this connection, we revert to our map, where the visible spectrum has an extent in wave-lengths of 0.36^μ, then, on that same scale, the length of the entire possible spectrum, visible and invisible, was fixed by Draper at the point there shown near the band $\rho\sigma\tau$. In still other words, according to him the very end of any spectrum at all would be less than 3 on a scale in which the visible spectrum was 1. Doctor Draper's authority was deservedly respected, and this citation of his remarks is made only to show the view then entertained by eminent men of science.

Now, the writer had proved by actual measurement that it extended far beyond this point, and had announced, as the result of experiment, that it extended at any rate to about three times the utmost length then assigned from theoretical reasons by Draper, founded on the then universally accepted formula of Cauchy, which was later discredited by the direct experimental evidence given of its falsity by the bolometer.

The bolometer, which is wholly independent of light as a sensation and notes it only as a manifestation of energy, first lays down the spectrum by curves of energy from which the linear spectrum is in turn derived. Two such curves taken at different times are given to show the agreement. There must now be explained, however briefly, the way in which these energy curves, which are the basis of all, have actually been produced here.

In making the map of the energy curves it should be remembered that when an invisible band or line is suspected, its presence is revealed by the change of temperature in the bolometer strips affecting the needle of the galvanometer, causing this needle to swing this way or that; let us suppose to the left if from cold and the right if from heat. The writer's first method was to have one person to note the exposure, another to note the extent of the deflection, and a third to note the part of the spectrum in which it occurred. For reasons into which he does not enter, this old plan was tedious in the extreme and required, as has been said, hundreds of observations to fix with appropriate accuracy the position in wave-length of one invisible line. It has been stated that only about twenty such lines had been mapped out in nearly two years of assiduous work prior to 1881, and if a thousand such lines existed, it was apparent that fifty years would be required to denote them.

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[1] Proceedings of the American Academy, vol. xvi, p. 233, 1880.

The writer then devised a second apparatus to be used in connection with the bolometer. This apparatus was simple in theory, though it has taken a dozen years to make it work well in practice, but it is working at last, and with this the maps in this volume of the "Annals" and that before us have been chiefly made. It is almost entirely automatic, and as it is now used, a thousand inflections can be delineated in a single hour, much better than this could have been done in the half century of work just referred to.

Briefly, the method is this: A great rock-salt prism (for a glass one would not transmit these lower rays nor could they easily be detected in the overlapping spectra of the grating) is obtained of such purity and accuracy of figure, and so well sheltered from moisture, that its clearness and its indications compare favorably, even in the visible spectrum, with those of the most perfect prism of glass, with the additional advantage that it is permeable to the extreme infra-red rays in question. This prism rests on a large azimuth circle turned by clockwork of the extremest precision, which causes the spectrum to move slowly along, and in one minute of time, for example, to move exactly one minute of arc of its length before the strip of the bolometer, bringing this successively in contact with one invisible line and another. Since what is blackness to the eye is cold to the bolometer, the contact of the black lines chills the strip and increases the electric current. The bolometer is connected by a cable with the galvanometer, whose consequent swing to the right or the left is photographically registered on a plate which the same clockwork causes to move synchronously and uniformly up or down by exactly one centimeter of space for the corresponding minute. By this means the energy curve of an invisible region, which directly is wholly inaccessible to photography, is photographed upon the plate.

Let it be noted that whatever the relation of the movement of the spectrum to that of the plate is, (and different ones might be adopted), it is absolutely synchronous—at least to such a degree that an error in the position of one of these invisible lines can be determined, as has been stated, with the order of precision of the astronomical measurement of visible things.

The results were before them in the energy curves and the linear infra-red spectrum containing over seven hundred invisible lines. This is more than the number of visible ones in Kirchoff & Bunsen's charts. The position of each line is fixed from a mean of at least six independent determinations with the accuracy stated above.

The reader will perhaps gather a clearer idea of this action if he imagines the map before him hung up at right angles to its actual position, so that a rise in the energy curve given would be seen to correspond to a deflection to the right, and a fall, to one to the left; for in this way the deflections were written down on the moving photographic plate from which this print has been made. The writer was now speaking of the refinements of the most recent practice; but there was something in this retrospect of the instrument's early use which brought up a personal reminiscence which he asked the Academy to indulge him in alluding to.

This was that of one day in 1881, nearly twenty years ago, when being near the summit of Mount Whitney, in the Sierra Nevadas, at an altitude of 12,000 feet, he there, with this newly invented instrument, was working in this invisible spectrum. His previous experience had been that of most scientific men—that very few discoveries come with a surprise, and that they are usually the summation of the patient work of years.

In this case, almost the only one in his experience, he had the sensations of one who makes a discovery. He went down the spectrum, noting the evidence of invisible heat die out on the scale of the instrument until he came to the apparent end even of the invisible, beyond which the most prolonged researches of investigators up to that time had shown nothing. There he watched the indications grow fainter and fainter until they too ceased at the point where the French investigators believed they had found the very end of the end. By some happy thought he pushed the indications of this delicate instrument into the region still beyond. In the still air of this lofty region the sunbeams passed unimpeded by the mists of the lower earth, and the curve of heat, which had fallen to nothing, began to rise again. There was something there. For he found, suddenly and unexpectedly, a new spectrum of great extent, wholly unknown to science and whose presence was revealed by the new instrument, the bolometer.

This new spectrum is given on the map, where it will be observed that while the work of the photograph (much more detailed than that of the bolometer, where it can be used at all) has been stated to extend, as far as regular mapping is concerned, to about 1.1^{μ}, that everything beyond this is due to the bolometer, except that early French investigators had found evidence of heat extending to 1.8^{μ}. Still beyond that ultima thule, this region, which he has ventured to call the "New Spectrum," extends. It will be found between wave-lengths 1.8^{μ} and 5.3^{μ} on the map.

The speaker had been much indebted to others for the perfection to which the apparatus, and especially the galvanometer, had been brought. He was under obligations, particularly to Mr. Abbot, for assistance in many ways, which he had tried to acknowledge in the volume; but before closing this most inadequate account of it, he would like to draw attention to one feature which was not represented in the spectrum map before them, although it would be found in the book.

During early years the impression had been made upon him that there were changes in the spectrum at different periods of the year. Some of these changes might be in the sun itself. The major portion of those he was immediately speaking of, he believed, were rather referable to absorptions in the earth's atmosphere.

Now these early impressions had been confirmed by the work of the observatory in recent years, and charts given in the volume would show that, (the sun being always supposed to be at about the same altitude, and its rays to traverse about the same absorbing quantity of the earth's atmosphere), the energy spectrum was distinctly different in spring, in summer, in autumn, and in winter. The lateness of the hour prevented him from enlarging on this latter profoundly interesting subject. He would only briefly point out the direction of these changes, which were not perhaps to be called conspicuous, but which seemed to be very clearly brought out as certainly existing. With regard to them he would only observe, what all would probably agree to, that while it has long been known that all life upon the earth, without exception, is maintained by the sun, it is only recently that we seem to be coming by various paths, and among them by steps such as these, to look forward to the possibility of a knowledge which has yet been hidden to us, of the way in which the sun maintains it. We were hardly beginning to see yet how this could be done, but we were beginning to see that it might later be known, and to see how the seasons, which wrote their coming upon the records of the spectrum, might in the future have their effects upon the crops prevised by means somewhat similar to those previsions made day by day by the Weather Bureau, but in ways infinitely more far-reaching, and that these might be made from the direct study of the sun.

We are yet, it is true, far from able to prophesy as to coming years of plenty and of famine, but it is hardly too much to say that recent studies of others as well as of the writer, strongly point in the direction of some such future power of prediction.

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SMITHSONIAN MISCELLANEOUS COLLECTIONS.

1258

ON THE

CHEAPEST FORM OF LIGHT.

BY

S. P. LANGLEY

AND

F. W. VERY.

WASHINGTON CITY:
PUBLISHED BY THE SMITHSONIAN INSTITUTION.
1901.

WASHINGTON, D. C.
JUDD & DETWEILER, PRINTERS
1901

ON THE CHEAPEST FORM OF LIGHT.

FROM STUDIES AT THE ALLEGHENY OBSERVATORY.*

BY

S. P. LANGLEY AND F. W. VERY.

The object of this memoir is to show by the study of the radiation of the fire-fly that it is possible to produce light without heat other than that in the light itself; that this is actually effected now by nature's processes, and that these are cheaper than our industrial ones in a degree hitherto unrealized. By "cheapest" is here meant the most economical in energy, which for our purpose is nearly synonymous with heat; but as a given amount of heat is producible by a known expenditure of fuel at a known cost, the word "cheapest" may also here be taken with little error in its ordinary economic application.

We recall that in all industrial methods of producing light there is involved an enormous waste, greatest in sources of low temperature, like the candle, lamp, or even gas illumination, where, as I have already shown, it ordinarily exceeds 99 parts in the 100; and least in sources of high temperature, like the incandescent light and electric arc, where yet it is still immense and amounts even under the most favorable conditions to very much the larger part.

It has elsewhere† been stated that for a given expense at least one hundred times the light should in theory be obtainable which we actually get by the present most widely used methods of illumination. This, it will be observed, is given as a minimum value, and it is the object of the present research to demonstrate that not only this possible increase, but one still greater, is actually obtained now in certain natural processes, which we know of nothing to prevent our successfully imitating.

It is now universally admitted that wherever there is light there has been expenditure of heat in the production of radiation existing in and as the luminosity itself, since both are but forms of the same energy; but this visible radiant heat which is inevitably necessary is not to be considered as waste. The waste comes from the present necessity of expending a great deal of heat in invisible forms before reaching even the slightest visible result, while each increase of the light represents not only the small amount of heat directly concerned in the making of the light itself, but a new indirect expenditure in the production of invisible calorific rays. Our eyes recognize heat mainly as it is conveyed in certain rapid ethereal vibrations associated with high temperatures, while we have no usual way of reaching these high temperatures without passing through the intermediate low ones, so that if the vocal production of a short atmospheric vibration were subject to analogous conditions, a high note could never be produced until we had passed through the whole gamut, from dicontinuous sounds below the lowest bass up successively through every lower note of the scale till the desired alto was attained.

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* Reprinted, with additional note, from the American Journal of Science, third series, vol. xl, No. 236, August, 1890.

† See results of an investigation by S. P. Langley, read before the National Academy in 1883, and given in "Science" for June 1, 1883, where it is shown that in the ordinary Argand burner gas flame indefinitely *over* 99 per cent of the radiant energy is (for illumination purposes) waste.

There are certain phenomena, long investigated, yet little understood, and grouped under the general name of "phosphorescent" which form an apparent exception to this rule, especially where nature employs them in the living organism, for it seems very difficult to believe that the light of a fire-fly, for instance, is accompanied by a temperature of 2,000°, or more, Fahr., which is what we should have to produce to gain it by our usual processes. That it is, however, not necessarily impossible, we may infer from the fact that we can by a known physical process produce a still more brilliant light without sensible heat, where we are yet sure that the temperature exceeds this. No sensible heat accompanies the fire-fly's light, any more than need accompany that of the Geissler tube, but this might be the case in either instance, even though heat were there, owing to its minute quantity, which seems to defy direct investigation. It is usually *assumed*, with apparent reason, that the insect's light is produced without the invisible heat that accompanies our ordinary processes, and this view is strengthened by study of the fire-fly's spectrum, which has been frequently observed to diminish more rapidly toward the red than that of ordinary flames.

Nevertheless, this, though a highly probable and reasonable assumption, remains assumption rather than proof, until we can measure with a sufficiently delicate apparatus the heat which accompanies the light and learn not only its quantity, but, what is more important, its quality. Apart from the scientific interest of such a demonstration is its economic value, which may be inferred from what has already been said. I have therefore thought it desirable to make the light of the fire-fly the subject of a new research, in which it is endeavored to make the bolometer supplement the very incomplete evidence obtainable from the visible spectrum.

As we may learn from elementary treatises, phenomena of phosphorescence are common to insects, fishes, mollusks, vegetables, and organic and mineral matter. Among luminous insects the fire-fly of our fields is a familiar example, though other of the species attain greater size, and perhaps greater intrinsic brilliancy, especially the *Pyrophorus noctilucus* Linn., found in Cuba and elsewhere. Its length is about 37^{mm}, width 11^{mm}, and it has, like Pyrophori, three light reservoirs—two in the thorax and one in the abdomen. To procure this Cuban fire-fly I invoked the aid of the Smithsonian Institution, and through the kindness of Professor Felipe Poey, of Havana, and Señor Albert Bonzon, of Santiago de Cuba in the Island of Cuba, living specimens of the *Pyrophorus noctilucus* were received here during the summer of 1889. I have also to acknowledge my obligations to Professor C. V. Riley and to Professor L. A. Howard, to whose knowledge and kind care I am doubly indebted.

After a preliminary spectral examination in Washington, I found it more convenient to continue the research at the Allegheny Observatory by means of the very special apparatus supplied by the liberality of the late William Thaw, of Pittsburgh, for researches in the lunar heat-spectrum.* Photometric measurements throughout the spectrum of the insect's light were also made.

I have indicated the steps of the investigation, but the experiments have been so largely and so intelligently made by Mr. F. W. Very that it is just to consider him as an associate rather than an assistant in the researches. I shall accordingly in what follows not discriminate between what each has contributed.

HISTORICAL NOTES.

We make no attempt to give any bibliography of the subject, and these notes are confined to what seems important in the history of the physical side of it.

Nathaniel Hulme.†—Exper. 6. A dead shining glow-worm was put upon water, contained in a wide-mounted phial, at the temperature of 58. The phial was then sunk in boiling hot water, and as the heat communicated itself to the contents of the phial, the light of the glow-worm became much more vivid.

Exper. 7. Another lucid dead glow-worm was put into warm water, at 114, to see if that degree of heat would extinguish the light, but, on the contrary, its glowing property was augmented. All the water was then poured off, yet the insect continued to shine for some length of time.

Exper. 8. Two living glow-worms were put into a one-ounce phial, with a glass stopple, and though they were perfectly dark at the time, yet if the phial was briskly rubbed with a silken or linen handkerchief till it became pretty warm, it seldom failed to make them display their light very finely. This experiment was very frequently repeated. It had the same illuminating effect upon the light of a dead glow-worm.

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* Described in the Memoirs of the National Academy, vol. iv, part ii, p. 112.

† Philos. Trans. Roy. Soc., London, vol. xc, pp. 180, 181, 1800.

Exper. 9. The complete influence of 212 degrees of heat was now applied to the light of a glow-worm, by pouring upon one when dead, but in a luminous state, some boiling water. Its light was instantly extinguished thereby and did not revive. The experiment was repeated and with the same result.

Macaire * (quoted by Becquerel) found that the luminous matter taken from the body of a glow-worm and heated, increased in brilliancy up to a temperature of about 41° C., after which the light diminished, became reddish, and ceased at 52° C. An electric current increased the luminosity in both the living insect and in the luminous part separated from the remainder of the body, but ceased to have any effect in a vacuum. Oxygen and carbon monoxide increased the light of the living insect and of the luminous matter taken from its body, but the light ceased in a vacuum, in hydrogen, in carbon dioxide, in sulphurous anhydride, and in sulphureted hydrogen.

Carus † observed that the luminous matter taken from the body of the glow-worm ceases to shine when dried, but glows again when moistened.

Matteucci ‡ found that the phosphorescent substance of the Italian glow-worm (*Lampyris Italica*) soon ceased to glow in hydrogen or in carbon dioxide, but shone decidedly brighter in oxygen than in air, the oxygen being consumed and carbon dioxide appearing. He drew the conclusion that the production of light in this insect is entirely due to the combination of oxygen with carbon, which is one of the elements of the phosphorescent matter. The greatest brilliancy occurred at a temperature of 37° or 38° Cent., but all phosphorescence ceased above 50° or below — 6° Cent.

Robert § found that a glow-worm cut in halves continued to glow for half an hour, when the light ceased, but commenced again on the near approach of a candle, and continued as bright as ever for thirty-six hours, after which it was impossible to renew it.

Pasteur ‖ has examined the spectra of our Pyrophorus without finding any appearance of bright or dark lines. He states that M. Gernez has made a similar observation on the spectrum of the glow-worm.

Becquerel ¶ gives a good summary of the results of previous observers. Since phosphorescent solids give banded spectra and thus differ from ignited solids and liquids which have continuous spectra, M. Becquerel concludes, from the apparent continuity of the spectrum of the light from phosphorescent animals, that their light approaches nearer to that of ordinary incandescence—a deduction which the following result renders unnecessary.

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* "Bibliotheque Univ. de Genève," 1821.

† "Analecten zer Natur- und Heilkunde," Leipzig, 1829; see also Comtes Rendus, lix, p. 607, 1864.

‡ "Ann. de chim. et de phys.," III, ix, p. 71, 1843; also in C. R., xvii, p. 309.

§ C. R., xvii, p. 627, 1843.

‖ C. R., lix, p. 509, 1864.

¶ "La lumiere," 1867.

C. A. Young * states that the "common" fire-fly gives a continuous spectrum, extending from a little above Fraunhofer's line C in the scarlet, to about F in the blue, gradually fading out at the extremities. He observes that it is noticeable that precisely this portion of the spectrum is composed of rays, which, while they more powerfully than any others affect the organs of vision, produce hardly any thermal or actinic effect. In other words, very little of the energy expended in the flash of the fire-fly is wasted.

(This is a most important and interesting inference, but it will be observed that this is necessarily rather assumed as highly probable than actually demonstrated, since the method did not permit the dealing with the invisible rays except by inference.)

It is quite different with our artificial methods of illumination. In the case of an ordinary gas light, experiments show that at most, one per cent of the radiant energy consists of *visible* rays, the rest being invisible heat; that is to say, over ninety-nine per cent of the gas is wasted in producing rays that do not help in making objects visible. †

Secchi ‡ at first thought that the spectrum of the glow-worm was monochromatic, but with an improved spectroscope, recognized that other colors were present, though feebly, and decided that the spectrum was sensibly continuous.

Quatrefages,§ in connection with the paper of Secchi, remarks that the previous observations of Spallanzani and Macaire, repeated with much care by Matteucci and Becquerel, show beyond doubt that the light of glow-worms and elaters is due to slow combustion. Thus the light is extinguished in a vacuum, and in irrespirable gases, it reappears in contact with the air, it is perceptibly increased by the presence of pure oxygen, it persists after the death of the creature, and finally it is accompanied by the generation of carbon dioxide. Nevertheless, he points out that there is a distinct kind of phosphorescence in the marine Noctilucidæ, due to the contraction of muscular fiber, the shining tissue being seen through the translucent body wall. This species of phosphorescence is increased by irritants, but is independent of the presence of oxygen and is not extinguished or in any way modified by hydrogen or by carbon dioxide.

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* The American Naturalist, Salem, 1870, vol. iii, p. 615.

† S. P. Langley has shown that the waste is in fact even greater than this; see "Science," vol. i, No. 17, p. 482, 1883.

‡ C. R., lxxv, p. 321, 1872.

§ C. R., lxxv, p. 322, 1872.

Robin and *Laboulbene*[*] find the luminous organs of *P. noctilucus* composed of irregularly polyhedral cells, $0{\cdot}04^{\mathrm{mm}}$ to $0{\cdot}06^{\mathrm{mm}}$ thick, between which pass very numerous fine tracheæ and nerves. The inner face of the organ is composed of adipose tissue, and the outer of a transparent modification of the ordinary chitinous covering of the insect. The authors conclude that the light is due to chemical decomposition of a nitrogenous body with formation of crystalline urates.

Jousset de Bellesme[†] finds that although the phosphorescent cells, when separated from the body of the insect, continue to glow for several hours, yet if crushed they instantly lose their illuminating power, which indicates that for the production of the light the living cells must retain their integrity, and that they are not mere reservoirs of a phosphorescent substance, but continuous generators of it. He surmises that the light-giving substance may be phosphureted hydrogen.

Meldola[‡] is quoted by Spiller[§] as having examined the glow-worm spectrum and determined its approximate limits.

Conroy[||] finds the glow-worm's light green, and in a small direct vision spectroscope showing a continuous spectrum from C to *b*, appearing like a broad band of green light extending from $0^{\mu}{\cdot}518$ to $0^{\mu}{\cdot}587$ with a faint continuous spectrum into the red to $0^{\mu}{\cdot}656$.

R. Du Bois.[¶]—Perhaps the most important of previous memoirs on phosphorescent insects is by this writer. It contains an account of photometric measures in wave-length scale, and also of heat measures with the thermopile. The latter represent the only attempt even, in this direction, I know of, and seem to be judiciously made but to be insufficient (on account of the limitations of such apparatus) to establish the author's conclusion that the light is accompanied by no sensible heat. This conclusion, we repeat, though very probably correct, does not seem to rest on the evidence of an apparatus of at all the necessary sensitiveness. This memoir, however, appears to be in general an excellent one, and well worthy the student's attention.

From all these statements it is abundantly clear that not only physicists and chemists, but naturalists, have been led to conclude that this light is not associated indissolubly with any so-called vital principle or vital process, but it is a result of certain chemical combinations, and that nothing forbids us to suppose it may be one day produced by some process of the laboratory or manufactory. With this conclusion in mind, we now proceed to observations meant to demonstrate the fact that this process (presumably discoverable but still unknown) gives light without invisible heat.

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[*] C. R., lxxvii, p. 511, 1873.

[†] C. R., xc, p. 318, 1880.

[‡] "Proc. Entomological Soc.," p. iii, 1880.

[§] "Nature," vol. xxvi, p. 343.

[||] "Nature," vol. xxvi, p. 319, 1882.

[¶] "Bulletin de la Société Zoologique de France," parts 1, 2, and 3, 1886.

These observations are: 1. Photometric. 2. Thermal.

PART 1.—PHOTOMETRIC OBSERVATIONS.

The first impression on viewing the light of the *Pyrophorus noctilucus* through a spectroscope is that it consists essentially of a broad band in the green and yellow, while with precaution we see this extending into and beyond the borders of the blue and orange, but not very greatly farther, and these have been taken by previous observers as its absolute limits. No one appears to have experimentally and distinctly answered the question, "Would the light not extend farther were it bright enough to be seen?" nor has it been proven as clearly as might be desired that the result depends on the quality rather than the quantity of the light, or given conclusive evidence, that if the light of the insect were as bright as that of the sun it would not extend equally far on either side of the spectrum.

It is impossible to increase the intrinsic brilliancy by any optical device, but if it be impossible to make the light of the insect as bright as that of the sun, it is on the other hand quite possible to make the light of the sun no brighter than that of the insect, and this would appear to be the first step in obtaining a definite proof that the apparently narrow limits of the insect's spectrum are due to the intrinsic quality of the light and not to its feeble intensity. The only conclusive method of determining this would appear to be to balance the light from the insect with that of a definite portion of sunlight by any ordinary photometric device; and having taken this sunlight as nearly equal as possible to that of the insect, though certainly not greater, to let this determined quantity fall on the slit of a spectroscope at the same time with the light from the insect, two spectra being formed one over the other in the same field and at the same time.

The actual doing this is not so easy as it might appear, owing to experimental difficulties connected with the insect, a part of which arises from the fact that its light is not only fitful but unequal, being of very varying intensities when not wholly intermittent.

The simplest way in which the experiment can be performed is perhaps the following:

The insect is placed immediately in front of the slit of a spectroscope, so that the light of its thoracic or abdominal portion falls upon the slit. This forms a narrow spectrum which should be brought into the lower or upper half of the field, the insect being attached to the spectroscopic apparatus in a position as nearly fixed as possible. The spectroscope is now placed with the axis of its collimator in the line of a ray of sunlight cast from a heliostat without. In the path of this ray is a screen with a circular diaphragm covered with ground glass; a lens in front of the slit casts on one portion of it an image of the white circle formed by the ground glass, which image is the same size as the illuminating organ of the insect and forms a spectrum of the same height in the reserved portion of the field. A suitable disposition of lenses placed between the glass screen and the siderostat enables any degree of illumination to be given to the former, from full sunlight to nearly absolute darkness. If the normal spectrum be studied, a grating is selected of such open ruling that the entire visible spectrum of the first order can be seen in the field, but the grating is first so placed that what is seen is not the spectra but the reflected image of the slit, the grating thus acting (at first) the part of a mirror; so that the observer first sees the two circles of light of approximately equal size and brilliancy, one formed by the insect, the other by the sunlight, and the light of this latter, by the arrangement of lenses between the screen and the siderostat is then adjusted so that while remaining of the size of the insect, it is judged to have the same intrinsic brilliancy, or, at any rate, not a superior one.

The essential thing is that a photometric comparison shall be made of the two lights before the spectra are formed, and that under these conditions the sunlight is equal but not superior to that of the insect.

The necessary condition of equality of the two lights from which the spectra are to be formed, having thus been secured, the grating is moved until the two spectra are brought into the field. The result of this direct test is that the solar spectrum when intrinsically of the same brightness, or even when clearly of less brightness than that of the insect, extends somewhat further toward the red and distinctly further toward the violet, the insect light being more intense than that of the sun for equal lights in the green, but ending more abruptly on the violet side.

It may be added that when the insect's light grew brighter the increment appeared to be more in the blue end or as if the average wavelength diminished with the intensity, but there was not opportunity to put this beyond doubt.

Photometric observations in the prismatic spectrum were made previously to the adoption of the arrangement above detailed, the first being on July 1, 1889, using thoracic light. The insect was mounted on an adjustable stand, to which it was attached loosely, so as to give it such freedom of motion as is needed to insure its emitting the light. It was consequently necessary to readjust its position incessantly, and this necessity constitutes a very obvious difficulty. The thoracic light spots are two ovals, each about 2^{mm} by $1{\cdot}5^{mm}$ (see plate I, Fig. 1). Their light is not so bright as the abdominal light, but much steadier, and like that, of a decidedly greenish hue. One of these oval spots was placed over the center of a slit, open just enough to receive the light, or about $1{\cdot}5^{mm}$. This slit was in the focus of a glass lens of 8^{cm} aperture and 82^{cm} focus, which acted as a collimator. The prism was a very large one of flint (faces $11\cdot5^{cm}$ high, $10\cdot5^{cm}$ wide), whose mounting included an automatic minimum deviation attachment. The observing lens was similar to the collimator, with a low-power eye-piece in whose field was a pair of heavy vertical parallel wires. The whole was mounted on the spectrometer, primarily designed for bolometric measures and fully described elsewhere.* The insect turned so as to show the abdominal light is depicted in plate I, Fig. 2.

The observer waited for some time in a wholly darkened room, and to the eye thus rendered sensitive, the visible spectrum, before magnification, was about 2^{mm} high and 20^{mm} long, the parallel wires being distinctly visible in the indigo at a setting of 45° 25′, corresponding to a wave-length of $0^{\mu}\cdot468$, and in the red at 43° 53′, corresponding to $0^{\mu}\cdot640$. The spectrum then was visible from a little beyond F to near C, or through a range of $0^{\mu}\cdot172$. As might have been anticipated from the greenish color of the light, the maximum brilliancy was in the green near E, or near wave-length $0^{\mu}\cdot53$.† From this point the light fell away on both sides more rapidly than in the solar spectrum. (See plate II, A, B.)

July 2. A comparison of the spectra of the thoracic and of the abdominal light gave the latter upon the average about double the intrinsic brightness of the former. This was only a crude estimate, but more exact methods under the limited time for experiment would have been useless, owing to the very fluctuating character of the light. In continuation of the photometric measurements of the preceding day on the thoracic light, this was compared with that from the flame of an ordinary Bunsen burner at its greatest luminosity, whose area was limited by a diaphragm to that of the size of the thoracic light. The light from the base of this luminous flame (height of flame about $3\cdot5^{cm}$, air shut off at base of burner) gave a continuous spectrum, which in these first comparisons was alternated with that of the insect. The spectra were judged to be equal in the blue and the red, but that of the insect was much brighter in the green. Again, a spectrum being formed from light taken midway between the base and point of the flame was found to be everywhere too bright, but especially so in the red.

July 3. Continuation of photometric measures, but with abdominal light. (The abdominal luminous organ is shown in plate I, Fig. 2.)

				μ	
Wires seen in	indigo	45°	29′	0·463	Abdominal light.
" "	red	43	47	0·663	Range $0^{\mu}\cdot200$.
" "	indigo	46	56	0·390	Range $0^{\mu}\cdot382$.
" "	red	43	21	0·772	Bunsen burner.

(Luminous flame 4^{cm} high, at point one-third down from top, just within inner and slightly darker cone, seen through hole $2\cdot5^{mm}$ in diameter.)

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*American Journal of Science, March, 1883, p. 188.

† In the normal spectrum the maximum has a wave-length $0^{\mu}\cdot57$.

Under these circumstances the spectrum of the insect's light was in the green a fair match for that of the burner; elsewhere the latter was brighter, but not very greatly so. Since the insect's spectrum was followed through $0^{\mu}{\cdot}18$ with the thoracic light, while with that of the same character, but double the brightness, it was followed only through a very little more, or $0^{\mu}{\cdot}20$, and while at the same time that of a but slightly brighter artificial flame was followed through nearly double or $0^{\mu}{\cdot}38$, it seems probable that the insect's light actually ceases near the given limits, and does not merely disappear from the inability of the eye to follow a diminishing light. While we observe from these first photometric measures that the insect's spectrum has undoubtedly a decided maximum in the green, we are led to infer that this spectrum is very probably of the nature of a broad band stretching from beyond F to near C *where it terminates*, and this very important inference we shall see confirmed later by other and more exact measures.

August 5. Comparison of relative brightness in different parts of spectrum of abdominal light with that from a student's lamp.

A spectrometer supplied with means* for bringing into the same field the spectra of two different lights, formed by a Rutherfurd grating of 17296 lines to the inch (instead of the prism) was employed for this purpose. The upper half of the slit received the insect's light, the lower half a beam from the brightest part of the Argand flame, which had passed through two Nicol's prisms, one of which was attached to a divided circle. The two spectra were then seen in the same field with their edges in exact juxtaposition. In the field of the observing telescope was a slit 1^{mm} wide, subtending not quite 9·5 (minutes of arc), which allowed light having a range of wave-length of about $0^{\mu}{\cdot}01$ to pass. The spectrum of the lamp-light was brighter in every part of the field though in unequal degrees till it was diminished by turning the Nicol's prism. The angle through which the prism was turned to produce equality having been noted, the values deduced from the ordinary formula (transmitted light $= l \cos^2 a$, the angle a being 90° when the light is diminished as much as possible by crossing the planes of the Nicols at right angles) are as follows, where the fractions are those by which the brightness of the lamp spectrum at the various points is to be multiplied to produce equality with the insect spectrum.

Part of spectrum corresponding to center of slit at focus of observing telescope.	Blue green very near F.	Green near *b*.	Green near E.	Yellow green.	Citron.	Yellow.	Orange yellow near D.	Orange.
	µ	µ	µ	µ	µ	µ	µ	µ
Wave length......	0·49	0·51	0·53	0·54	0·56	0·58	0·59	0·60
Brightness.........	0·02	0·21	0·34	0·37	0·24	0·19	0·17	0·09

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*Alluded to but not fully described in the American Journal of Science, August, 1877.

Owing to the motion of the insect and the varying brilliancy of the light emitted, these figures (each of which is the result of the mean of several trials including at least two measures) still leave much to be desired. The supply of the insects which had been procured and maintained alive with difficulty, however, did not allow of the experiments being further prolonged, nor of the securing a direct comparison with the solar spectrum. The value of each part of the lamp spectrum having, however, been independently determined with all possible exactness in terms of the solar spectrum, we are enabled to exhibit a comparison of the latter with the insect spectrum so as to show them together (plate II, Figs. A and B). It is assumed that the same amount of luminous intensity (*i. e.*, energy in terms of vision as determined by purely photometric methods) is taken, whether from the sun or the insect. The subjoined curves (plate II) show the solar and the insect luminosity throughout the visible spectrum on the preceding assumption of the intrinsic equality, a result which, however, might be liable to a slight correction of the relative places of the maxima if a direct comparison with sunlight were obtained. The important fact, however, seems to be brought out almost beyond question that when spectra are formed from two *equal* lights, one from the sun, the other from the insect, the latter's spectrum terminates both at an upper and a lower limit at which the solar light is still conspicuous. The conclusion follows that the insect spectrum is lacking in the rays of red luminosity and presumably in the infra-red rays, usually of relatively great heat, or that it seems probable that we have here *light without heat*, other than that heat which the luminosity itself comprises and which is but another name for the same energy.

Any other supposition would apparently involve the hypothesis that the spectrum, which we have seen end at the red, has a renewal in the invisible infra-red where the main portion of the solar heat and that of all ordinary illuminants is known to exist. Although this last hypothesis cannot be considered to have much weight, and though we are led to agree with previous observers that it may be assumed with much probability that the ordinary invisible heat would, if we had means to observe it, be found unassociated with the fire-fly's light, yet this assumption is itself far from being proof, and in view of the great importance of the conclusions in question, we shall now try whether it be possible to settle the point by thermal measures with the bolometer.

PART 2.—THERMAL OBSERVATIONS.

To give an idea of the amount of heat at our disposition for experiment, and of the actual minuteness of the radiation which proceeds from even the most luminous tropical insect, we may say that if that rate of radiation from a lamp-black surface 1 square cm. in area, which represents the amount of heat necessary to raise 1 gram of water, 1° Centigrade, in 1 minute (*i. e.*, one small calorie) be taken as unity, then the luminous radiation of the fire-fly's heat, per square cm. of exposed luminous surface, as we have found, is about 0·0004 calorie in 10^{sec}, and the total luminous radiation from the most powerfully illuminating light spot of the insect (the abdominal one) will not exceed 0·00007 calorie in the same time. But a small portion of this could fall upon the bolometer, and that which actually reached it during the time (10^{sec}) required for each observation, was sufficient only to affect an ordinary mercurial thermometer having a bulb 1^{cm} in diameter by rather less than $0^{\circ}\cdot0000023$, or by less than $\frac{1}{400000}$ of one degree centigrade.

We have just mentioned that the total amount of heat radiation upon which we have to make our investigation represents less than $\frac{7}{100000}$ calorie, while that portion of this which falls upon the apparatus would, in the time of one operation, only raise the temperature of an ordinary mercurial thermometer by less than $\frac{1}{400000}$ degree, and we have first to notice the difficulty that in case invisible heat exists in company with the light (and it certainly does exist in ordinary emanations from the surface of any living creature independent of phosphorescence) we have, in this minute radiation, heat of two different kinds, both invisible and which it is yet indispensable for us to discriminate.

We are helped to do this by the consideration that while the insect, like any non-luminous one, must emit " animal heat " from all its surface, its general surface temperature is certainly low, since it feels cold to the hand whose greater warmth excites it to shine. This heat then corresponds to a temperature much below 50° Cent., and such temperatures must, as we have shown in other memoirs, be accompanied by the emission of waves whose length relegates them to quite another spectral region to that in which the invisible heat associated with light mainly appears. We can then discriminate the rays of this invisible " animal " heat without the formation of a heat spectrum by their inability to pass through a glass which transmits with comparative freedom radiant heat whose wave-length is less than 3^{μ}, the latter including the region where, if there be invisible heat radiated with the light, it must mainly lie.

The heat in the spectral region of the infra-red we are considering, we know in advance must be, if it bear any sort of relation to the light, almost immeasurably small, and in fact it defied at first all attempts to obtain not merely a quantitative measurement, but even any certain experimental evidence of its existence. At last, upon July 24, with the arrival of a new stock of over two dozen insects and with the aid of experience derived from previous failures, these heat measures were resumed. For the first described, the thoracic light is taken.

The insect was placed 125^{cm} from the mirror of $25{\cdot}4^{\text{cm}}$ aperture and $73{\cdot}4^{\text{cm}}$ focus, so that its image was formed at 178^{cm} and enlarged about 1.42 diameters, when a small portion of it filled an aperture equal to the bolometer employed, which was selected from the most sensitive of those used in previous researches in lunar heat and had an aperture of $19^{\text{sq mm}}$. By the preceding arrangement of the mirror an image of one of the thoracic bright spots, with enough of the surrounding body to represent an area of about $13^{\text{sq mm}}$, was enlarged to nearly the surface of the bolometer.

Employing all the precautions taught by a multiplied experience, we obtained by a series of exposures of the bolometer to the insect radiation a series of small but real galvanometer deflections which represent the excess of total heat radiations from the insect over those from a metal plate of a temperature of about 25° C., forming the background. These heat radiations come jointly from the luminous spot (area 3 to $4^{\text{sq mm}}$) and about $9^{\text{sq mm}}$ of the surrounding body. To determine their characters we interposed a sheet of glass * which cut off all the observed heat. The heat from the luminous spectrum and from a spectral region below it extending to about 3^{μ} (30,000 tenth meters) was known to be capable of passing through this glass. The evidence then is that there is no heat in the spectrum below this feeble radiation from the luminous thoracic region, sufficient to be capable of affecting the apparatus, though this was so sensitive as to promptly respond to the feeble body radiation from the somewhat larger section of the luminous and non-luminous surface.

Continuation on Abdominal Heat.

The insect's light then is unaccompanied (in the specimen subject to this experiment) by any measurable heat, but to make it still more evident that this is due to the absence of heat below the red (body heat not being in question) we now proceed to take an artificial flame, occupying the same area as the radiating luminous part of the insect, and to see whether heat is observed in it. If the flame be no brighter than the insect, and the heat be nevertheless observed in it when in the insect heat is lacking, it is obvious that in the latter case none is observed because (sensibly) none is emitted, and this conclusion is reached *a fortiori* when the flame light is less than that of the insect.

July 27. Through a circular aperture $2{\cdot}5^{\text{mm}}$ in diameter there was passed alternately the total radiant heat, and that transmitted by glass from a nearly non-luminous Bunsen flame, whose luminosity was very much *fainter* than that from the insects. On this day there seemed to be an exceedingly minute deflection averaging ¼ of one division of the galvanometer scale from the total radiation of an equal portion of the abdominal light spot of the insect, while from the flame there was a mean deflection of 177·5 divisions, showing that the total heat radiation from an equal area of a less luminous flame was many hundred times that from the luminous area of the insect.

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* Described in the Memoir "On the Temperature of the Surface of the Moon," Mem. Nat. Acad. of Sciences, vol. iii, as "B."

Glass being interposed, the heat due to this flame radiation fell to 14·5 divisions, or about 8 per cent of the original radiation, showing that of the quality of Bunsen flame heat immediately in question (that above 3^{μ} transmissible by glass) there was still something like 60 times that of the combined body and luminous radiation of the insect in the far less luminous flame. Subsequently, by the use of a lens giving greater concentration, measurable indications of insect radiation above 3^{μ}, and therefore distinct from any possible body heat, were obtained through glass, showing the flame radiation of this quality from an equal area of the same intrinsic brilliancy—*i. e.*, *invisible* heat and of long wave-length, but shorter than 3^{μ}—to be about 400 times that of the insect.

These experiments were repeated with different luminous flames and with different insects on succeeding days. In some of them especially luminous insect specimens were secured, which, with favorable conditions of the galvanometer, gave very measurable deflections on the latter. By a similar use of the glass to that described, it appeared that flames whose intrinsic brilliancy is nearly comparable to that of a point below the middle of the candle flame, and whose total brilliancy is as exactly as possible comparable to that of the insect, give several hundred times the heat of the latter, even if we consider only that quality of heat which is found above 3^{μ}, while if we compare the total radiations (*i. e.*, those directly observed without the use of the glass) the contrast is still stronger.

It follows that the insect light is accompanied by approximately one-four-hundredth part of the heat which is ordinarily associated with the radiation of flames of the luminous quality of those which were the subject of experiment. This value is confirmed by other methods which we do not give here. It will conduce to a clearer comprehension of this if we exhibit in a series of curves derived from our observations the spectral distribution of one unit of energy in the gas-flame spectrum (plate III, Fig. 1); of the electric arc spectrum (plate III, Fig. 2); of the sun (plate V, Fig. 3), and of the insect (plate III, Fig. 4). In all these the abscissæ are the same, the portion between $0^{\mu}.4$ and $0^{\mu}.7$ (violet to red) showing the part of the energy utilized in light, while that from $0^{\mu}.7$ to 3^{μ} shows the part wasted as invisible heat. The energy in each case being the same, the areas are the same, except that owing to the relative importance of the light heat curve (Fig. 4) only about $\frac{1}{20}$ of the latter can be shown in the limits of the plate.

The curves in plate II deal with luminous intensity only, and give no means of drawing those economic conclusions which appear to follow from our experiments and which the curves in plate III supply. These curves (plate III) all exhibit the spectrum on the normal scale, from that easily visible, lying between $0^{\mu}.4$ in the violet and $0^{\mu}.7$ in the red, then to 3^{μ} near the limit of the glass transmission. In the case of the first three, representing spectra of the gas flame, the electric arc, and the sun, nearly all the energy lies above 3^{μ}. In that of the gas flame a considerable portion lies below 3^{μ} (and still more in that of the candle flame, if that were shown where most of the energy would lie below 3^{μ} or outside the limits of the drawing). The curves, then, we repeat, represent equal amounts of energy (which without sensible error we may assume to be all exhibited as *heat*) and inclose equal areas.

The total area represents in each case the expenditure of a unit of cost in thermal energy, the area between $0^{\mu}\cdot 4$ and $0^{\mu}\cdot 7$ the proportion of this utilized as *light*, though, as we have just stated, in the case of Fig. 4, the representative of the fire-fly spectrum, only a fraction of this can be shown (owing to the limits of the drawing).

Resuming, then, what we have said, we repeat that nature produces this cheapest light at about one-four-hundredth part of the cost of the energy which is expended in the candle flame, and at but an insignificant fraction of the cost of the electric light or the most economic light which has yet been devised, and that, finally, there seems to be no reason why we are forbidden to hope that we may yet discover a method (since such a one certainly exists and is in use on the small scale) of obtaining an enormously greater result than we now do from our present ordinary means for producing light.

APPENDIX.

Determination in Calories of the Heat in the Luminous (Abdominal) Radiation of Pyrophorus noctilucus.

The determination is reached by two steps: (1) The calibration of the galvanometer, so as to give the value of its division in calories, and (2) the inference from the observed deflection in divisions of the total of calories radiated.

1. The bolometer, whose face occupied 0·19 sq. cm. (a), gave a deflection of 342 divisions (b) at a distance of 25 cm. (r) from a 5 cm. circular aperture filled by a blackened Leslie cube. Seen from the center of this aperture, the bolometer occupied, then $\frac{a}{2\pi r^2} = 0{\cdot}0000484$ of the hemisphere, and would have received this fraction of the total radiation, except that being placed exactly opposite the radiating surface, more than the mean radiation fell on it in a proportion which calculation shows to be about $\frac{1}{3}$. The fraction of the total radiation which it actually received, then, was 0·0000645 (c).

Accordingly the total radiation would have caused a deflection $\frac{b}{c} = 5300000$ divisions.

The surface of the cube was at a temperature of 99° Cent., and was limited by the diaphragm to an area of 19·6 sq. cm. (d). The total radiation from one centimeter, then, would have caused a deflection of

$\frac{b}{cd} = 270400$ div. The temperature of the bolometer, which was that of the apartment, was 20° C. According to Dulong and Petit's law, the radiation from such a surface at 99° C. to one at 20° C. would be 1·11 cal. per minute (e), which does not greatly differ from our own independent determinations, and for $10^{\text{sec.}} = \overset{\text{min.}}{0{\cdot}167}$ (f) (the time of the galvanometer swing) it equals $\overset{\text{cal.}}{0{\cdot}185}$ (ef). Hence $\frac{b}{cdef} = \frac{270400}{0{\cdot}185} = 1462000$ div. is the potentiality of work in 1 calorie, to be expressed in the swing of the galvanometer needle, and 1 div. $= \overset{\text{cal.}}{0{\cdot}000000684}$.

2. The galvanometer received the fire-fly radiation through a lens which occupied 0·00655 of a hemisphere, and would have transmitted this fraction of the total heat, except for its position, which caused it to transmit ⅓ more than the average, which is 0·00873 (g). The measured radiation from this fractional part gave $\overset{\text{div.}}{0{\cdot}84}$ div. (h) and $\frac{h}{g} = 96{\cdot}2$ div. is the deflection which would be given by the total abdominal emission, or

$$96{\cdot}2 \times 0{\cdot}000000684 = \overset{\text{cal.}}{0{\cdot}0000658}.$$

Since the luminous surface has an area of about ⅙ sq. cm., this corresponds to a radiation of $\overset{\text{cal.}}{0{\cdot}00039}$ per sq. cm. of radiating surface in the time of the galvanometer needle's swing, or to $\frac{0{\cdot}0004}{f} = \overset{\text{cal.}}{0{\cdot}0024}$ per sq. cm. per minute.

(Taking the water-equivalent of the bulb of an ordinary mercurial thermometer 1 cm. in diameter at $\overset{\text{gr.}}{0{\cdot}25}$ we find

$$\frac{0{\cdot}84 \times {\cdot}000000684}{0{\cdot}25} = 0^{\circ}{\cdot}0000023,$$

showing that if such a thermometer were placed in the position occupied by the bolometer its rise during the time of the latter's exposure to the radiation of the insect would be between two and three one-millionths of a centigrade degree.)

NOTE, 1901.

These notes, added to the preceding article with the assistance of Dr. H. C. Bolton, are simply intended to point out some of the principal additions to our knowledge of the subject which have appeared during the last ten years; and they make no pretension to be in any sense a bibliography.

About one year after publication of the preceding paper Dr. William H. Seaman, of Washington, wrote an article " On the Luminous Organs of

Insects" (Proc. Am. Soc. Micros., 14th meeting), in which he claims that the emission of light by these insects is connected with the process of oxidation. He says: "In several species of luminous insects the structure of the photogenic organs is specially adapted to secure the most copious supply of air throughout the substance of the gland," and he concludes that this peculiar structure is to enable the glands to produce light by the action of oxygen, the energetic principle of the air.

Lophin ($C_{21}H_{16}N_2$) exhibits this phenomenon at the low temperature of 10° C., in the presence of oxygen and alkalies.

The identity of the light from living beings and that from carbon compounds has been established by spectroscopic observation. The caustic alkali, which is incompatible with organic life, can be replaced by cholin, neurin, and other bodies that are known constituents of living beings.

The quantity of these materials required to produce light is very small; 1.82 grams of lophin dissolved in 25cc. strong alcoholic potash gave light for 20 days throughout its entire mass.

The Russian experimenter extracted with ether from the bodies of 180 *Pelagia noctiluca* a thick yellow liquid that shone beautifully on shaking with an alkali.

In 1893 Raphael Dubois,* whose monograph, "Les Elaterides lumineux" (Bull. Soc. Zoölog., 1886) has been cited, published a paper recording his observations on a myriapod from Algeria, *Orya barbarica* (C. R., cxvii, 184). He confirmed the statement of M. Gàzagnaire, who had observed (in May, 1888) that the phosphorescent substance is secreted by these insects on the sternal and episternal plates as a yellowish viscid liquid. The excretion is distinctly acid, a fact that disproves, he says, the oxidation theory, and he gives to the substance the name "luciferine."

Peter Schmidt, of St. Petersburg, published a paper in Russian "On the luminosity of Midges," which was reproduced in 1895, in the Annals and Magazine of Natural History (series 6, vol. 25, p. 133). Schmidt divides luminous insects into two classes: A, those provided with special luminous organs, and, B, those having luminous micro-organisms living upon them. He places midges in the latter group.

In 1896 H. Muraoka examined lampyrid beetles collected in Kyoto, Japan, and found they emitted both actinic and Röntgen rays; he noted reflection of the rays, but was unable to determine refraction, interference, and polarization. Muraoka found the entire body actinically active, and to increase the light sprinkled the insects with water; he also observed that the dead beetles were luminous when kept moist and when rubbed. (Wied. Ann. Phys., vol. 59, p. 773.)

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*See also paper by Dubois on Physiological Light in Smithsonian Report for 1895.

No attempt is here made at a complete bibliography, but the following papers may be consulted by the reader:

C. Verhoeff; Zur Biologie von *Phosphœnus hemipterus* und Verwandten. Verh. Ver. Rheinland, 1894, 208.

W. L. Distant; (Luminosity of Fulgoridæ.) Trans. Entomol. Soc. London, 1895, 429.

A. S. Packard; Phosphorescence of Organs of Insects. J. N. Y. Entomol. Society, 1896, 61.

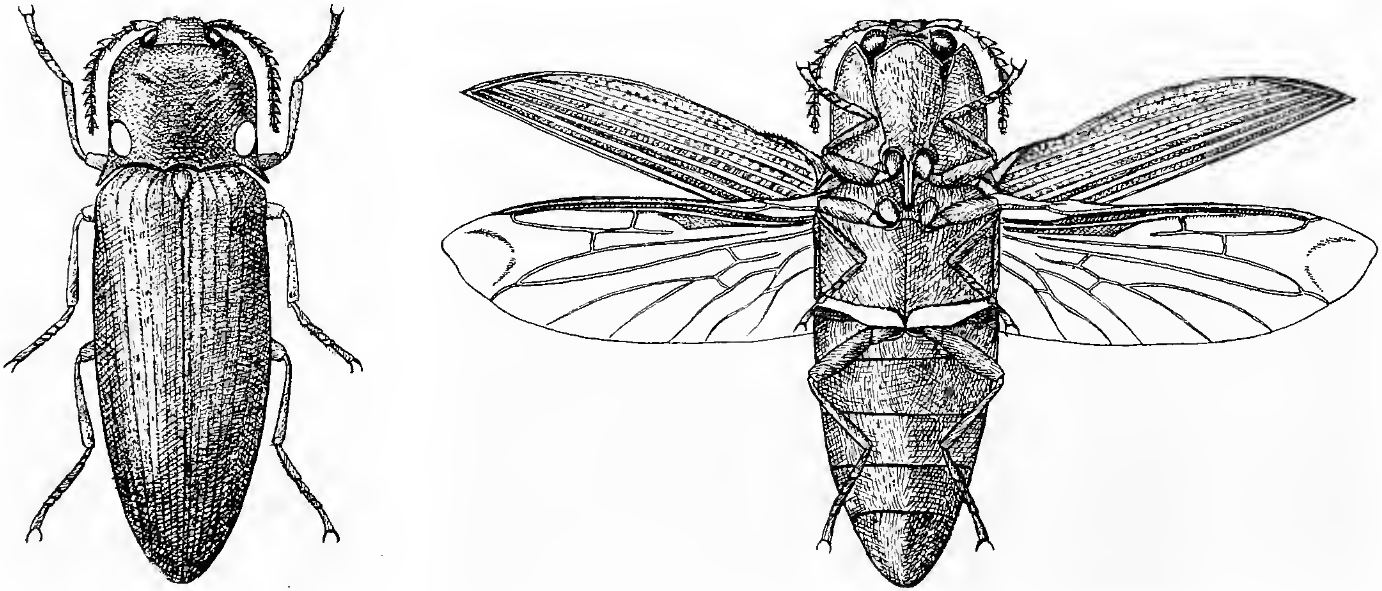
R. Dubois; Les œufs lumineuses et leurs larves. Leçons de physiologie generales et comp. xii, Paris, 1898, p. 301.

Besides these biologic sources of light, there have been discovered several kinds of mineral matter possessing the extraordinary power of emitting visible, actinic, and skiagraphic rays. Becquerel, in 1896, announced that all salts of uranium, whether fluorescent or not, yield invisible radiations capable of discharging electrified bodies and of producing skiagraphic images on photographic plates; and this property persisted even after long confinement in a double leaden box.

Two years later Mme. Sklodowska Curie, of Paris, having examined a series of uranium minerals, found in pitchblende a substance emitting Becquerel rays, so called, 4,000 times stronger than uranium compounds, and named the element believed to be contained therein "polonium." A few months later M. and Mme. Curie announced a second body having similar properties; this they called "radium." Not long after, a third element, "actinium," was added to the list. Radium, which is the best known, is analogous to barium and accompanies it in chemical reactions so closely that the two have not been satisfactorily separated.

I have, with Dr. H. C. Bolton, verified the fact that the rays given out by small amounts of radium compounds not previously insolated yield, under favoring circumstances, photographic transparencies very clear in details; they also exert chemical action, and they excite fluorescence in barium-platino-cyanide screens even through plates of vulcanite, aluminium, etc., like Röntgen rays. These radio-active bodies have been studied by physicists and chemists for several years, but without success, in isolating the elements or in demonstrating the true source of the energy manifested.

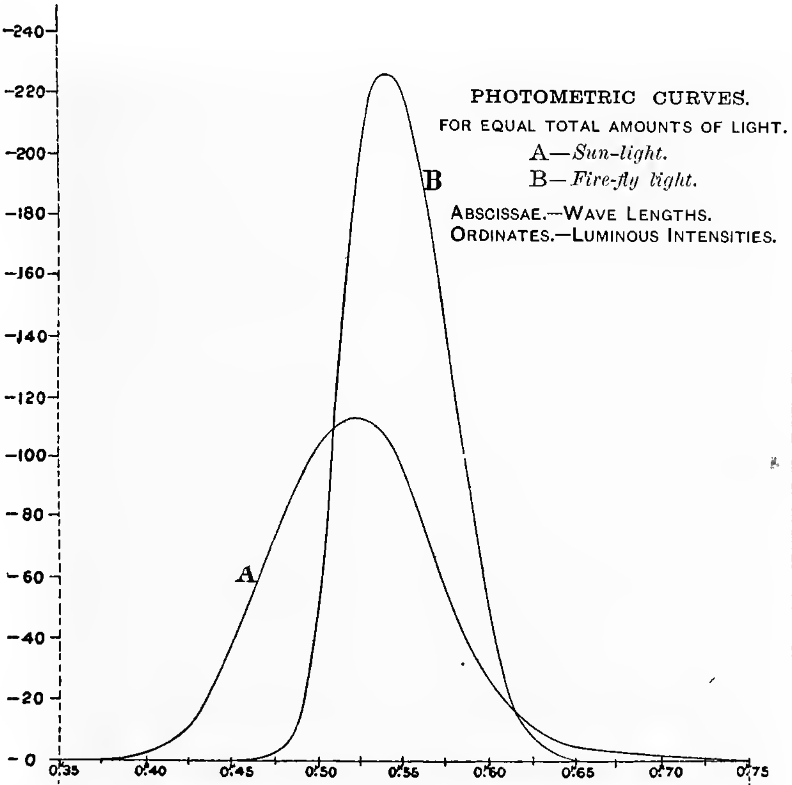
PLATE I

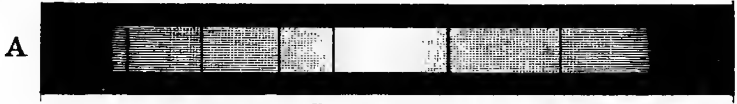


PYROPHORUS NOCTILUCUS

TWICE NATURAL SIZE

PLATE II





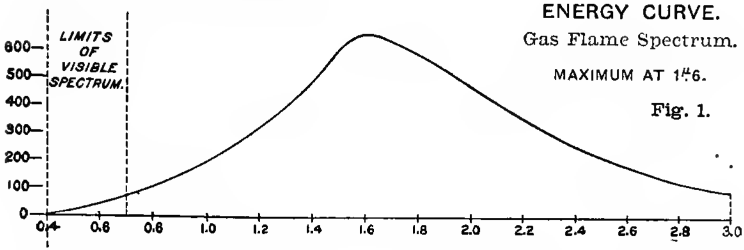
A. ***Sun-light Spectrum.***

B. ***Fire-fly Spectrum.***

PLATE III

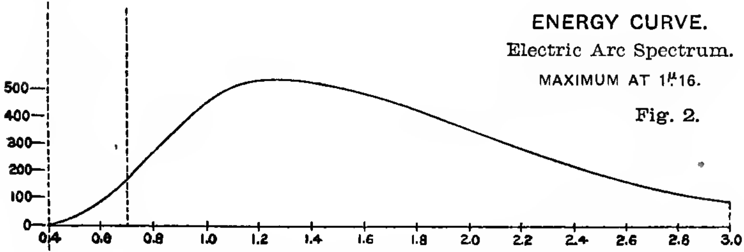
Four Curves of Equal Areas, showing one unit of heat displayed successively in heat spectrum of Gas, Electric Arc, Sun and Fire-Fly.

ABSCISSAE. — WAVE LENGTHS.
ORDINATES. — ENERGY AS HEAT.



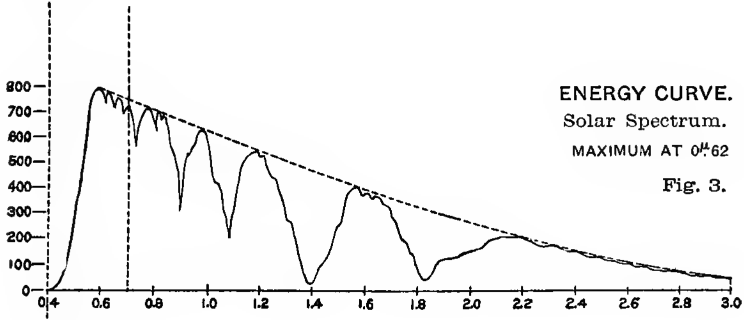
ENERGY CURVE.
Gas Flame Spectrum.
MAXIMUM AT 1$^{\mu}$.6.

Fig. 1.



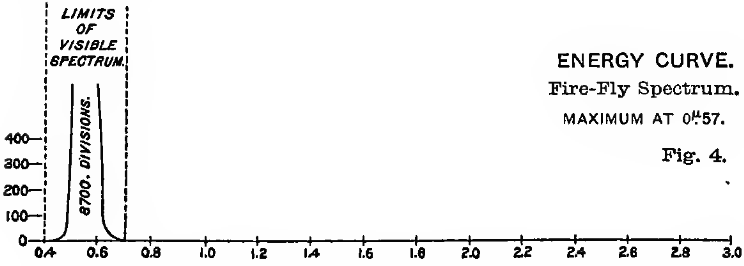
ENERGY CURVE.
Electric Arc Spectrum.
MAXIMUM AT 1$^{\mu}$.16.

Fig. 2.



ENERGY CURVE.
Solar Spectrum.
MAXIMUM AT 0$^{\mu}$.62

Fig. 3.



ENERGY CURVE.
Fire-Fly Spectrum.
MAXIMUM AT 0$^{\mu}$.57.

Fig. 4.

— 1269 —

THE LANGLEY AERODROME.

FROM THE SMITHSONIAN REPORT FOR 1900, PAGES 197–216
(WITH PLATES I–VI).

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1901.

THE LANGLEY AËRODROME.

I. NOTE PREPARED FOR THE CONVERSAZIONE OF THE AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, NEW YORK CITY, APRIL 12, 1901.

What is popularly known as the "flying machine" is literally a machine, without gas to support it, in no way resembling a balloon, and which its inventor has called the Aërodrome. The Aërodrome (from words signifying "air runner") is, then, the name given to this apparatus by Mr. Langley to indicate the principle of its action, which in no way resembles that of a balloon that floats, because it is lighter than the air, while the aërodrome is hundreds of times heavier than the air. The weighty machine owes its support to another principle—that is, to the *rapidity* with which it runs over the air, like a skater on thin ice. The balloon in a calm remains indefinitely suspended over one spot. This machine, built almost entirely of steel, is far heavier in relation to the air than a ship of solid lead would be in relation to the water, and could not remain in the air if still.

The essence of its action, then, is in its motion, without which it could not remain suspended. It is moved rapidly by a steam engine, carrying its own fuel and its water supply, by which it can be kept up indefinitely, while it is also, and by the necessity of its own action, rapidly advancing.

This may all be admitted as probably true in theory, but it is not generally known that this has actually been done.

The two large photographs are each about one-third the full size of one of several working models,[1] each of which is driven by a steam engine of over 1¼ horsepower. This and other like models have repeatedly flown distances of over half a mile, at a speed of from 20 to 30 miles an hour.

This actual result has not been advertised, and is comparatively little known, though these models are believed to have done something absolutely new in the history of the world. They are the product of a great many years of assiduous labor, and represent the condition of the experiments in Mr. Langley's hands up to the close of the year 1896, since which time he has made no public statement of his work, which is understood to be still going on in connection with experiments for the War Department in demonstrating the possible uses of the future aërodrome as an engine of war.

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[1] Here shown in reduced size, Plates IV, V.

Reverting to the present models, they represent a machine whose weight is about 30 pounds, one-fourth of which is contained in the engine and machinery, which is of unexampled lightness. Within the small body, seen in the photograph suspended under the main rod, is contained everything for generating 1¼ (brake) horsepower, the total weight of fire grate, boiler, and every accessory being less than 7 pounds. The engine, with its cylinders, pistons, and every moving part, weighs 26 ounces. This puts in motion the propellers, which, turning at a rate of between 800 and 1,200 revolutions per minute, drives the aërodrome at a speed which varies greatly, according to the inclination given to the motionless "wings."

Mr. Langley, after a great many years of preliminary experiment on supporting surfaces, which he has described in his "Experiments in aërodynamics," first made a remarkable, and to the engineer, most paradoxical statement; namely, that in such aërial navigation as was there shown to be possible, under certain definite conditions the power required would in theory *diminish* indefinitely as the speed increased, and that it would actually diminish in practice up to a certain limit.

This statement, which has since been called "Langley's law," is justified in practice, but the conditions which give this increase of speed with decrease of power are limited by others which demand that the flight should be made in safety and without that danger of accident which might come in applying rigorously exact theoretical conclusions without regard to the security of the flight. The actual speed which was obtained, then, was under conditions where security was chiefly sought.

In the experiments which have hitherto been made, safety has accordingly been the first consideration, and the "wings," or rather the motionless supporting surfaces, have been given such an inclination as to cause the speed to be limited to between 20 and 30 miles an hour. The machine has actually traveled very much faster than this, but its higher speeds have not been measured.

The aërodrome was launched from a specially constructed house boat on the Potomac in a secluded spot about 30 miles below Washington, and was supplied with water for a short course lest it should, in its uncontrolled flight, go altogether out of reach, and lose itself in the neighboring Virginia forests. The idea of making the flight over water from a house boat or raft may appear obvious when once stated, but like many simple results it was only reached after long experiment with other methods, and its utility has since been shown by its employment by others. There was no other reason why it should not fly for an indefinite time except the waste of water, which in the model had no provision for its renewal by condensation. This aërodrome, which is one of several which have flown considerable distances, performed this first flight on May 6, 1896, at a private trial of which Dr.

Alexander Graham Bell was the only witness. His contemporary statement may be found in the Comptes Rendus of the French Institute, CXXII, May 26, 1896.

A similar statement by him in the pages of Nature, May 28, 1896, vol. 54, is as follows:

> Through the courtesy of Mr. S. P. Langley, Secretary of the Smithsonian Institution, I have had on various occasions the privilege of witnessing his experiments with aërodromes, and especially the remarkable success attained by him in experiments made on the Potomac River on Wednesday, May 6, which led me to urge him to make public some of these results.
>
> I had the pleasure of witnessing the successful flight of some of these aërodromes more than a year ago, but Professor Langley's reluctance to make the results public at that time prevented me from asking him, as I have done since, to let me give an account of what I saw.
>
> On the date named two ascensions were made by the aërodrome, or so-called "flying machine," which I will not describe here further than to say that it appeared to me to be built almost entirely of metal and driven by a steam engine, which I have understood was carrying fuel and a water supply for a very brief period, and which was of extraordinary lightness.
>
> The absolute weight of the aërodrome, including that of the engine and all appurtenances, was, as I was told, about 25 pounds, and the distance from tip to tip of the supporting surfaces was, as I observed, about 12 or 14 feet.
>
> The method of propulsion was by aërial screw propellers, and there was no gas or other aid for lifting it in the air except its own internal energy.
>
> On the occasion referred to the aërodrome at a given signal started from a platform about 20 feet above the water and rose at first directly in the face of the wind, moving at all times with remarkable steadiness, and subsequently swinging around in large curves of perhaps a hundred yards in diameter, and continually ascending until its steam was exhausted, when, at a lapse of about a minute and a half and at a height which I judged to be between 80 and 100 feet in the air, the wheels ceased turning, and the machine, deprived of the aid of its propellers, to my surprise did not fall, but settled down so softly and gently that it touched the water without the least shock, and was in fact immediately ready for another trial.
>
> In the second trial, which followed directly, it repeated in nearly every respect the actions of the first, except that the direction of its course was different. It ascended again in the face of the wind, afterwards moving steadily and continually in large curves accompanied with a rising motion and a lateral advance. Its motion was, in fact, so steady that I think a glass of water on its surface would have remained unspilled. When the steam gave out again it repeated for a second time the experience of the first trial when the steam had ceased, and settled gently and easily down. What height it reached at this trial I can not say, as I was not so favorably placed as in the first, but I had occasion to notice that this time its course took it over a wooded promontory, and I was relieved of some apprehension in seeing that it was already so high as to pass the tree tops by 20 or 30 feet. It reached the water one minute and thirty-one seconds from the time it started, at a measured distance of over 900 feet from the point at which it rose.
>
> This, however, was by no means the length of its flight. I estimated from the diameter of the curve described, from the number of turns of the propellers, as given by the automatic counter, after due allowance for slip, and from other measures, that the actual length of flight on each occasion was slightly over 3,000 feet. It is at least safe to say that each exceeded half an English mile.
>
> From the time and distance it will be noticed that the velocity was between 20 and 25 miles an hour, in a course which was constantly taking it "up hill." I may add

that on a previous occasion I have seen a far higher velocity attained by the same aërodrome when its course was horizontal.

I have no desire to enter into detail further than I have done, but I can not but add that it seems to me that no one who was present on this interesting occasion could have failed to recognize that the practicability of mechanical flight had been demonstrated.

ALEXANDER GRAHAM BELL.

No adequate pictures have been made of the actual flight of the aërodrome, which from the rapidity of its motion required very special preparation; but Dr. Bell made on the uniquely interesting occasion of the first flight some photographs with a small pocket camera, from which pictures have been taken. They are necessarily inadequate as pictures, but they distinctly exhibit the aërodrome as a distant and elevated object in the air. (Pl. VI.)

II. PAPER FROM M'CLURE'S MAGAZINE.

[To partly satisfy a public curiosity which could not be altogether gratified, Mr. Langley wrote a wholly popular and untechnical account of the work which had gone on up to June, 1896, in McClure's Magazine. By the courtesy of the publishers he has been enabled to make considerable extracts from this article, which are reprinted here.

Attached to the present paper is an entirely mechanical reproduction of an instantaneous photograph (Plate VI) taken by Dr. Alexander Graham Bell, showing the aërodrome in actual flight, and which has never before been published. The original was taken with a small pocket camera and has been very greatly enlarged for the present article. It is necessarily inadequate, considered as a picture, but it is uniquely interesting as giving a distinct exhibit of the aërodrome as a distant elevated object in the air. The woods beneath it are the trees on the secluded island of Chopawamsic, near Quantico, on the Virginia shore of the Potomac River about 30 miles below Washington, where the flight occurred on May 6, 1896.]

THE "FLYING MACHINE."[1]

* * * Nature has made her flying machine in the bird, which is nearly a thousand times as heavy as the air its bulk displaces, and only those who have tried to rival it know how inimitable her work is, for the "way of a bird in the air" remains as wonderful to us as it was to Solomon, and the sight of the bird has constantly held this wonder before men's eyes and in some men's minds, and kept the flame of hope from utter extinction, in spite of long disappointment. I well remember how, as a child, when lying in a New England pasture, I watched a hawk soaring far up in the blue, and sailing for a long time

without any motion of its wings, as though it needed no work to sustain it, but was kept up there by some miracle. But, however sustained, I saw it sweep, in a few seconds of its leisurely flight, over a distance that to me was encumbered with every sort of obstacle, which did not exist for it. The wall over which I had climbed when I left the road, the ravine I had crossed, the patch of undergrowth through which I had pushed my way—all these were nothing to the bird, and while the road had only taken me in one direction, the bird's level highway led everywhere, and opened the way into every nook and corner of the landscape. How wonderfully easy, too, was its flight! There was not a flutter of its pinions as it swept over the field, in a motion which seemed as effortless as that of its shadow.

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[1] Reprinted, by permission, from McClure's Magazine, June, 1897. See also Story of Experiments in Mechanical Flight, by S. P. Langley, in Smithsonian Report, 1897, pp. 169–181.

After many years and in mature life, I was brought to think of these things again, and to ask myself whether the problem of artificial flight was as hopeless and as absurd as it was then thought to be. Nature had solved it, and why not man? Perhaps it was because he had begun at the wrong end, and attempted to construct machines to fly before knowing the principles on which flight rested. I turned for these principles to my books and got no help. Sir Isaac Newton had indicated a rule for finding the resistance to advance through the air, which seemed, if correct, to call for enormous mechanical power, and a distinguished French mathematician had given a formula showing how rapidly the power must increase with the velocity of flight, and according to which a swallow, to attain a speed it is now known to reach, must be possessed of the strength of a man.

Remembering the effortless flight of the soaring bird, it seemed that the first thing to do was to discard rules which led to such results, and to commence new experiments, not to build a flying machine at once, but to find the principles upon which one should be built; to find, for instance, with certainty by direct trial how much horsepower was needed to sustain a surface of given weight by means of its motion through the air.

Having decided to look for myself at these questions, and at first hand, the apparatus for this preliminary investigation was installed at Allegheny, Pa., about ten years ago. It consisted of a "whirling table" of unprecedented size, mounted in the open air, and driven round by a steam engine, so that the end of its revolving arm swept through a circumference of 200 feet, at all speeds up to 70 miles an hour. At the end of this arm was placed the apparatus to be tested, and, among other things, this included surfaces disposed like wings, which were hung from the end of the arm and dragged through the air till its resistance supported them as a kite is supported by the wind. One of the first things observed was that if it took a certain strain to sustain a properly disposed weight while it was stationary in the air, then not only to suspend it but to advance it rapidly at the same time took less strain than in the first case. A plate of brass weighing 1 pound, for instance, was hung from the end of the arm by a spring, which was drawn out till it registered that pound weight when the arm was still. When the arm was in motion, with the spring pulling the plate after it, it might naturally be supposed that, as it was drawn faster, the pull would be greater, but the contrary was observed, for under these circumstances the spring *contracted* till it registered less than an ounce. When the speed increased to that of a bird, the brass plate seemed to float on the air, and not only this, but taking into consideration both the strain and the velocity, it was found that absolutely less power was spent to make the plate move fast than slow, a result which seemed very extraordinary, since in all methods of land and water transport a high speed costs much more power than a slow one for the same distance.

These experiments were continued for three years, with the general conclusion that by simply moving any given weight of this form fast enough in a horizontal path it was possible to sustain it with less than one-twentieth of the power that Newton's rule called for. In particular it was proved that if we could insure horizontal flight without friction, about 200 pounds of such plates could be moved through the air at the speed of an express train and sustained upon it, with the expenditure of 1 horsepower, sustained, that is, without any gas to lighten the weight, or by other means of flotation than the air over which it is made to run, as a swift skater runs safely over thin ice, or a skipping stone goes over water without sinking, till its speed is exhausted. This was saying that, so far as power alone was concerned, mechanical flight was theoretically possible with engines we could then build, since I was satisfied that boilers and engines could be constructed to weigh less than 20 pounds to the horsepower, and that 1 horsepower would, in theory at least, support nearly ten times that if the flight were *horizontal.* Almost everything, it will be noticed, depends on this; for if the flight is downward it will end at the ground, and if upward the machine will be climbing an invisible hill, with the same or a greater effort than every bicycler experiences with a real one. Speed, then, and this speed expended in a horizontal course, were the first two requisites. This was not saying that a flying machine could be started from the ground, guided into such flight in any direction, and brought back to earth in safety. There was, then, something more than power needed; that is, skill to use it, and the reader should notice the distinction. Hitherto it had always been supposed that it was wholly the lack of mechanical power to fly which made mechanical flight impossible. The first stage of the investigation had shown how much, or rather how little, power was needed in theory for the horizontal flight of a given weight, and the second stage, which was now to be entered upon, was to show first how to procure this power with as little weight as possible, and, having it, how by its means to acquire this horizontal flight in practice; that is, how to acquire the *art* of flight or how to build a ship that could actually navigate the air.

One thing which was made clear by these preliminary experiments, and made clear nearly for the first time, was that if a surface be made to advance rapidly, we secure an essential advantage in our ability to support it. Clearly we want the advance to get from place to place; but it proves also to be the only practicable way of supporting the thing at all, to thus take advantage of the inertia of the air, and this point is so all-important that we will renew an old illustration of it. The idea in a vague sense is as ancient as classical times. Pope says:

> Swift Camilla scours the plain,
> Flies o'er the unbending corn, and skims along the main.

Now, is this really so in the sense that a Camilla, by running fast enough, could run over the tops of the corn? *If* she ran fast enough, yes; but the idea may be shown better by the analogous case of a skater who can glide safely over the thinnest ice if the speed is sufficient.

Think of a cake of ice of any small size, suppose a foot square. It possesses (like everything else in nature) inertia or resistance to displacement, and this will be less or more according to the mass moved. If the skater stands during a single second upon this small mass it will sink under him until he is perhaps waist deep in the water, while a cake of the same width but twice the length will yield only about half as readily to his weight. On this he will sink only to his knees, we may suppose, while if we think of another cake ten times as long as the first—that is, 1 foot wide and 10 feet long—we see that on this, during the same second, he will not sink above his feet. This is all plain enough; but now suppose the long cake to be divided into ten distinct portions, then it ought to be equally clear that the skater who glides over the whole in a second distributes his weight over just as much ice as though all ten were in one solid piece. So it is with the air. Even the viewless air possesses inertia; it can not be pushed aside without some effort; and while the portion which is directly under the airship would not keep it from falling several yards in the first second, if the ship goes forward so that it runs or treads on thousands of such portions in that time, it will sink in proportionately less degree; sink perhaps only through a fraction of an inch.

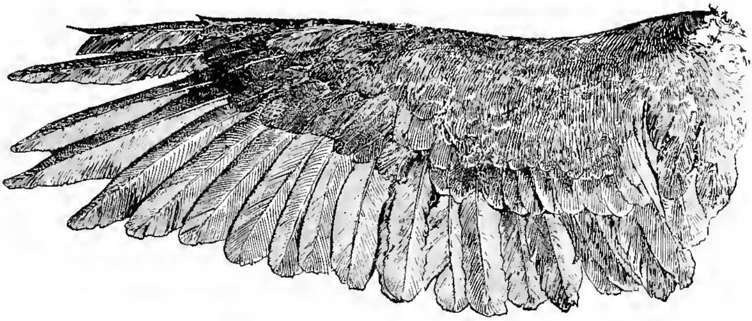
Speed, then, is indispensable here. A balloon, like a ship, will float over one spot in safety, but our flying machine must be in motion to sustain itself, and in motion, in fact, before it can even begin to fly.

Perhaps we may more fully understand what is meant by looking at a boy's kite. Everyone knows that it is held by a string against the wind, which sustains it, and that it falls in a calm. Most of us remember that even in a calm, if we run and draw it along it will still keep up, for what is required is motion relative to the air, however obtained.

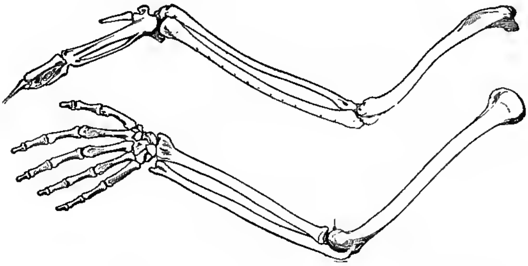
It can be obtained without the cord if the same pull is given by an engine and propellers strong enough to draw it and light enough to be attached to and sustained by it. The stronger the pull and the quicker the motion, the heavier the kite may be made. It may be, instead of a sheet of paper, a sheet of metal even, like the plate of brass which has already been mentioned as seeming, when in rapid motion, to float upon the air, and, if it will make the principle involved more clear, the reader may think of our aërodrome as a great steel kite made to run fast enough over the air to sustain itself, whether in a calm or in a wind, by means of its propelling machinery, which takes the place of the string.

And now, having the theory of the flight before us, let us come to the practice. The first thing will be to provide an engine of unprecedented lightness that is to furnish the power. A few years ago an engine that developed a horsepower weighed nearly as much as the actual horse did. We have got to begin by trying to make an engine which shall weigh, everything complete, boiler and all, not more than 20 pounds to the horsepower, and preferably less than 10; but even if we have done this very hard thing we may be said to have only fought our way up to an enormous difficulty, for the next question will be how to use the power it gives so as to get a horizontal flight. We must then consider through what means the power is to be applied when we get it, and whether we shall, for instance, have wings or screws. At first it seems as though nature must know best, and that since her flying models, birds, are exclusively employing wings, this is the thing for us; but perhaps this is not the case. If we had imitated the horse or the ox, and made the machine which draws our trains walk on legs we should undoubtedly never have done as well as with the locomotive rolling on wheels; or if we had imitated the whale, with its fins, we should not have had so good a boat as we now have in the steamship with the paddle wheels or the screw, both of which are constructions that nature never employs. This is so important a point that we will look at the way nature got her models. Here is a human skeleton, and here one of a bird, drawn to the same scale (Pl. I). Apparently nature made one out of the other, or both out of some common type, and the closer we look the more curious the likeness appears.

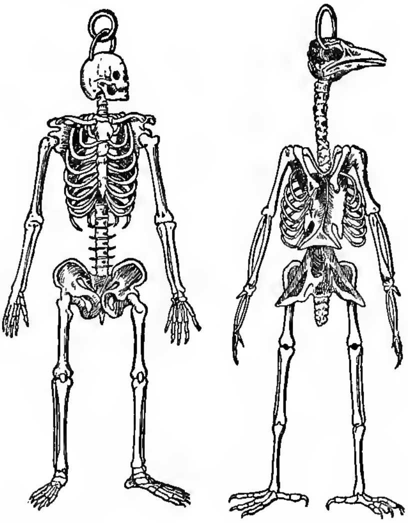
Here is a wing from a soaring bird, here the same wing stripped of its feathers, and here the bones of a human arm, on the same scale. Now, on comparing them, we see still more clearly than in the skeleton that the bird's wing has developed out of something like our own arm.



A WING FROM A SOARING BIRD.



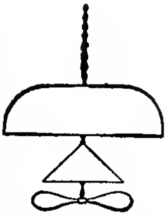
THE BONES OF A BIRD'S WING AND THE BONES OF A HUMAN ARM, DRAWN TO THE SAME SCALE, SHOWING THE CLOSE RESEMBLANCE BETWEEN THEM.



THE SKELETON OF A MAN AND THE SKELETON OF A BIRD, DRAWN TO THE SAME SCALE, SHOWING THE CURIOUS LIKENESS BETWEEN THEM.

First comes the humerus, or principal bone of the upper arm, which is in the wing also (Pl. I). Next we see that the forearm of the bird repeats the radius and ulna, or two bones of our own forearm, while our wrist and finger bones are modified in the bird to carry the feathers, but are still there. To make the bird, then, nature appears to have taken what material she had in stock, so to speak, and developed it into something that would do. It was all that nature had to work on, and she has done wonderfully well with such unpromising material; but anyone can see that our arms would not be the best thing to make flying machines out of, and that there is no need of our starting there when we can start with something better and develop that. Flapping wings might be made on other principles, and perhaps will be found in future flying machines, but the most promising thing to try seemed to me to be the screw propeller.

Some twenty years ago, Penaud, a Frenchman, made a toy, consisting of a flat, immovable, sustaining wing surface, a flat tail, and a small propelling screw. He made the wing and tail out of paper or silk, and the propeller out of cork and feathers, and it was driven directly by strands of india-rubber twisted lamplighter fashion, and which turned the wheel as they untwisted.


Penaud's flying toy (one-eighth of actual size).

The great difficulty of the task of creating a flying machine may be partly understood when it is stated that no machine in the whole history of invention, unless it were this toy of Penaud's, had ever, so far as I can learn, flown for even ten seconds; but something that will actually fly must be had to teach the art of "balancing."

When experiments are made with models moving on a whirling table or running on a railroad track, these are *forced* to move horizontally and at the same time are held so that they can not turn over; but in free flight there will be nothing to secure this, unless the air ship is so adjusted in all its parts that it tends to move steadily and horizontally, and the acquisition of this adjustment or art of "balancing" in the air is an enormously difficult thing, and which, it will be seen later, took years to acquire.

My first experiments in it, then, were with models like these, but from them I got only a rude idea how to balance the future aërodrome, partly on account of the brevity of their flight, which only lasted a few seconds, partly on account of its irregularity. Although, then, much time and labor were spent by me on these, it was not possible to learn much about the balancing from them.

Thus it appeared that something which could give longer and steadier flights than india rubber must be used as a motor, even for the preliminary trials, and calculations and experiments were made upon the use of compressed air, carbonic-acid gas, electricity in primary and storage batteries, and numerous other contrivances, but all in vain. The gas engine promised to be best ultimately, but nothing save steam gave any promise of immediate success in supporting a machine which would teach these conditions of flight by actual trial, for all were too heavy, weight being the great enemy. It was true also that the steam-driven model could not be properly constructed until the principal conditions of flight were learned, nor these be learned till the working model was experimented with, so that it seemed that the inventor was shut up in a sort of vicious circle.

However, it was necessary to begin in some way, or give up at the outset, and the construction began with a machine to be driven by a steam engine, through the means of propeller wheels, somewhat like the twin screws of a modern steamship, but placed amidships, not at the stern. There were to be rigid and motionless wings, slightly inclined, like the surface of a kite, and a construction was made on this plan which gave, if much disappointment, a good deal of useful experience. It was intended to make a machine that would weigh 20 or 25 pounds, constructed of steel tubes. The engines were made with the best advice to be got (I am not an engineer); but while the boiler was a good deal too heavy, it was still too small to get up steam for the engines, which weighed about 4 pounds, and could have developed a horsepower if there were steam enough. This machine, which was to be moved by two propelling screws, was labored on for many months, with the result that the weight was constantly increased beyond the estimate until, before it was done, the whole weighed over 40 pounds, and yet could only get steam for about a half horsepower, which, after deductions for loss in transmission, would give not more than half that again in actual thrust. It was clear that whatever pains it had cost, it must be abandoned.

This aërodrome could not then have flown; but having learned from it the formidable difficulty of making such a thing light enough, another was constructed, which was made in the other extreme, with two engines to be driven by compressed air, the whole weighing but 5 or 6 pounds. The power proved insufficient. Then came another, with engines to use carbonic-acid gas, which failed from a similar cause. Then followed a small one to be run by steam, which gave some promise of success, but when tried indoors it was found to lift only about one-sixth of its own weight. In each of these the construction of the whole was remodeled to get the greatest strength and lightness combined, but though each was an improvement on its predecessor, it seemed to become more and more doubtful whether it could ever be made sufficiently light, and whether the desired end could be reached at all.

The chief obstacle proved to be not with the engines, which were made surprisingly light after sufficient experiment. The great difficulty was to make a boiler of almost no weight which would give steam enough, and this was a most wearying one. There must be also a certain amount of wing surface, and large wings weighed prohibitively; there must be a frame to hold all together, and the frame, if made strong enough, must yet weigh so little that it seemed impossible to make it. These were the difficulties that I still found myself in after two years of experiment, and it seemed at this stage again as if it must, after all, be given up as a hopeless task, for somehow the thing had to be built stronger and lighter yet.

Now, in all ordinary construction, as in building a steamboat or a house, engineers have what they call a factor of safety. An iron column, for instance, will be made strong enough to hold five or ten times the weight that is ever going to be put upon it, but if we try anything of the kind here the construction will be too heavy to fly. Everything in the work has got to be so light as to be on the edge of breaking down and disaster, and when the breakdown comes all we can do is to find what is the weakest part and make that part stronger; and in this way work went on, week by week and month by month, constantly altering the form of construction so as to strengthen the weakest parts, until, to abridge a story which extended over years, it was finally brought nearly to the shape it is now, where the completed mechanism, furnishing over a horsepower, weighs collectively something less than 7 pounds. This does not include water, the amount of which depends on how long we are to run; but the whole thing, as now constructed, boiler, fire grate, and all that is required to turn out an actual horsepower and more, weighs something less than one one-hundredth part of what the horse himself does. I am here anticipating; but after these first three years something not greatly inferior to this was already reached, and so long ago as that, there had accordingly been secured mechanical power to fly, if that were all—but it is not all.

After that came years more of delay arising from other causes, and I can hardly repeat the long story of subsequent disappointment, which commenced with the first attempts at actual flight.

Mechanical power to fly was, as I say, obtained three years ago; the machine could lift itself if it ran along a railroad track, and it might seem as though, when it could lift itself, the problem was solved. I knew that it was far from solved, but felt that the point was reached where an attempt at actual free flight should be made, though the anticipated difficulties of this were of quite another order than those experienced in shop construction. It is enough to look up at the gulls or buzzards soaring overhead, and to watch the incessant rocking and balancing which accompanies their gliding motion, to apprehend that they find something more than mere strength of wing necessary, and that the machine would have need of something more than mechanical power, though what this something was was not clear. It looked as though it might need a power like instinctive adaptation to the varying needs of each moment, something that even an intelligent steersman on board could hardly supply, but to find what this was a trial had to be made. The first difficulty seemed to be to make the initial flight in such conditions that the machine would not wreck itself at the outset in its descent, and the first question was where to attempt to make the flight.

It became clear, without much thought, that since the machine was at first unprovided with any means to save it from breakage on striking against the ground, it would be well in the initial stage of the experiment not to have it light on the ground at all, but on the water. As it was probable that while skill in launching was being gained, and until after practice had made perfect, failures would occur, and as it was not desired to make any public exhibition of these, a great many places were examined along the shores of the Potomac and on its high bluffs which were condemned partly for their publicity, but partly for another reason. In the course of my experiments I had found out, among the infinite things pertaining to this problem, that the machine must begin to fly in the face of the wind and just in the opposite way to a ship, which begins its voyage with the wind behind it.

If the reader has ever noticed a soaring bird get upon the wing he will see that it does so with the breeze against it, and thus whenever the aërodrome is cast into the air it must face a wind which may happen to blow from the north, south, east, or west, and we had better not make the launching station a place like the bank of a river, where it can go only one way. It was necessary, then, to send it from something which could be turned in any direction, and taking this need in connection with the desirability that at first the airship should light in the water, there came at last the idea (which seems obvious enough when it is stated) of getting some kind of a barge or boat and building a small structure upon it which could house the aërodrome when not in use, and from whose flat roof it could be launched in any direction. Means for this were limited, but a little "scow" was procured, and on it was built a primitive sort of a house, one story high, and on the house a platform about 10 feet higher, so that the top of the platform was about 20 feet from the water, and this was to be the place of the launch (Pl. II). This boat it was found necessary to take down the river as much as 30 miles from Washington, where I then was—since no suitable place could be found nearer—to an island having a stretch of quiet water between it and the main shore; and here the first experiments in attempted flight developed difficulties of a new kind—difficulties which were partly anticipated, but which nobody would probably have conjectured would be of their actually formidable character, which was such as for a long time to prevent any trial being made at all. They arose partly out of the fact that even such a flying machine as a soaring bird has to get up an artificial speed before it is on the wing.

Some soaring birds do this by an initial run upon the ground, and even under the most urgent pressure can not fly without it.

Take the following graphic description of the commencement of an eagle's flight (the writer was in Egypt, and the "sandy soil" was that of the banks of the Nile):

> An approach to within 80 yards aroused the king of birds from his apathy. He partly opened his enormous wings, but stirs not yet from his station. On gaining a few feet more he begins to *walk* away with half-expanded, but motionless, wings. Now for the chance. Fire! A charge of No. 3 from eleven bore rattles audibly but ineffectively upon his densely feathered body; his walk increases to a run, he gathers speed with his slowly waving wings, and eventually leaves the ground. Rising at a gradual inclination, he mounts aloft and sails majestically away to his place of refuge in the Libyan range, distant at least 5 miles from where he rose. Some fragments of feathers denoted the spot where the shot had struck him. The marks of his claws were traceable in the sandy soil, as, at first with firm and decided digs, he forced his way; but as he lightened his body and increased his speed with the aid of his wings, the imprints of his talons gradually merged into long scratches. The measured distance from the point where these vanished to the place where he had stood proved that with all the stimulus that the shot must have given to his exertions he had been compelled to run full 20 yards before he could raise himself from the earth.

We have not all had a chance to see this striking illustration of the necessity of getting up a preliminary speed before soaring, but many of us have disturbed wild ducks on the water and noticed them run along it, flapping their wings for some distance to get velocity before they can fly, and the necessity of the initial velocity is at least as great with our flying machine as it is with a bird.

To get up this preliminary speed many plans were proposed, one of which was to put the aërodrome on the deck of a steamboat, and go faster and faster until the head wind lifted it off the deck. This sounds reasonable, but is absolutely impracticable, for when the aërodrome is set up anywhere in the open air we find that the very slightest wind will turn it over, unless it is firmly held. The whole must be in motion, but in motion from something to which it is held till that critical instant when it is set free as it springs into the air.

The house boat was fitted with an apparatus for launching the aërodrome with a certain initial velocity, and was (in 1893) taken down the river and moored in the stretch of quiet water I have mentioned—the general features of the place being indicated on the accompanying map, page 215—and it was here that the first trials at launching were made, under the difficulties to which I have alluded.

Perhaps the reader will take patience to hear an abstract of a part of the diary of these trials, which commenced with a small aërodrome which had finally been built to weigh only about 10 pounds, which had an engine of not quite one-half horsepower, and which could lift much more than was theoretically necessary to enable it to fly. The exact construction of this early aërodrome is unimportant, as it was replaced later by an improved one, of which a drawing is given on page 213, but it was the first outcome of the series of experiments which had occupied three years, though the disposition of its supporting surfaces, which should cause it to be properly balanced in the air and neither fly up nor down, had yet to be ascertained by trial.

What must still precede this trial was the provision of the apparatus for launching it into the air. It is a difficult thing to launch a ship, although gravity keeps it down upon the ways, but the problem here is that of launching a kind of ship which is as ready to go up into the air like a balloon as to go off sideways, and readier to do either than to go straight forward, as it is wanted to do, for though there is no gas in the flying machine, its great extent of wing surface renders it something like an albatross on a ship's deck—the most unmanageable and helpless of creatures until it is in its proper element.

If there were an absolute calm, which never really happens, it would still be impracticable to launch it as a ship is launched, because the wind made by running it along would get under the wings and turn it over. But there is always more or less wind, and even the gentlest breeze was afterwards found to make the air ship unmanageable unless it was absolutely clamped down to whatever served to launch it, and when it was thus firmly clamped, as it must be at several distinct points, it was necessary that it should be released simultaneously at all these at the one critical instant that it was leaping into the air. This is another difficult condition, but that it is an indispensable one may be inferred from what has been said. In the first form of launching piece this initial velocity was sought to be attained by a spring, which threw forward the supporting frame on which the aërodrome rested; but at this time the extreme susceptibility of the whole construction to injury from the wind and the need of protecting it from even the gentlest breeze had not been appreciated by experience. On November 18, 1893, the aërodrome had been taken down the river, and the whole day was spent in waiting for a calm, as the machine could not be held in position for launching for two seconds in the lightest breeze. The party returned to Washington and came down again on the 20th, and although it seemed that there was scarcely any movement in the air, what little remained was enough to make it impossible to maintain the aërodrome in position. It was let go, notwithstanding, and a portion struck against the edge of the launching piece, and all fell into the water before it had an opportunity to fly.

On the 24th another trip was made and another day spent ineffectively on account of the wind. On the 27th there was a similar experience, and here four days and four (round-trip) journeys of 60 miles each had been spent without a single result. This may seem to be a trial of patience, but it was repeated in December, when five fruitless trips were made, and thus nine such trips were made in these two months and but once was the aërodrome even attempted to be launched, and this attempt was attended with disaster. The principal cause lay, as I have said, in the unrecognized amount of difficulty introduced even by the very smallest wind, as a breeze of 3 or 4 miles an hour, hardly perceptible to the face, was enough to keep the airship from resting in place for the critical seconds preceding the launching.

If we remember that this is all irrespective of the fitness of the launching piece itself, which at first did not get even a chance for trial, some of the difficulties may be better understood; and there were many others.

During most of the year of 1894 there was the same record of defeat. Five more trial trips were made in the spring and summer, during which various forms of launching apparatus were tried with varied forms of disaster. Then it was sought to hold the aërodrome out over the water and let it drop from the greatest attainable height, with the hope that it might acquire the requisite speed of advance before the water was reached. It will hardly be anticipated that it was found impracticable at first to simply let it drop without something going wrong, but so it was, and it soon became evident that even were this not the case a far greater time of fall was requisite for this method than that at command. The result was that in all these eleven months the aërodrome had not been launched, owing to difficulties which seem so slight that one who has not experienced them may wonder at the trouble they caused.

Finally, in October, 1894, an entirely new launching apparatus was completed, which embodied the dozen or more requisites, the need for which had been independently proved in this long process of trial and error. Among these was the primary one that it was capable of sending the aërodrome off at the requisite initial speed, in the face of a wind from whichever quarter it blew, and it had many more facilities which practice had proved indispensable. (Pl. III.)

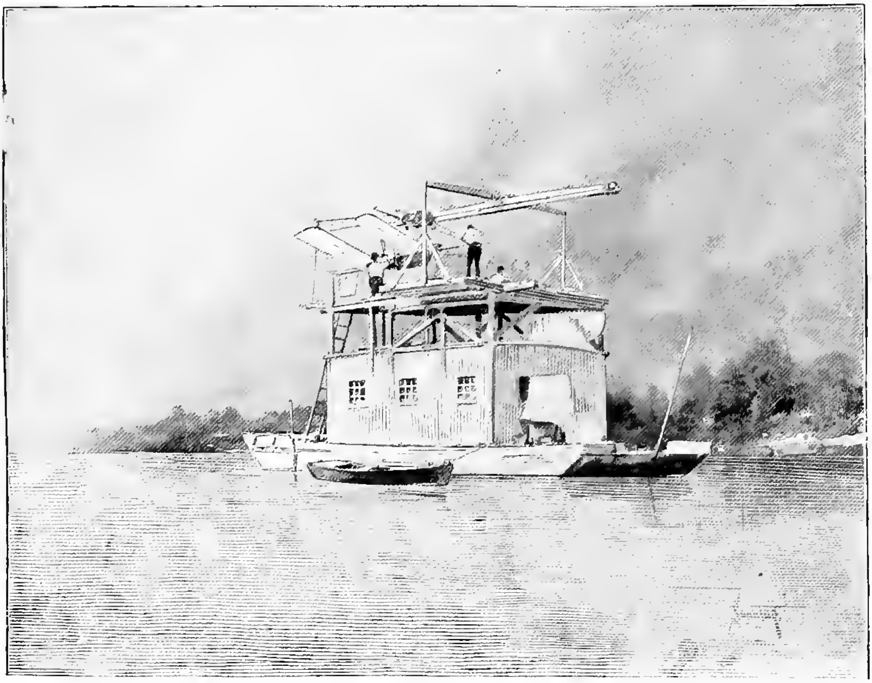
This new launching piece did its work in this respect effectively, and subsequent disaster was, at any rate, not due to it. But now a new series of failures took place, which could not be attributed to any defect of the launching apparatus, but to a cause which was at first obscure, for sometimes the aërodrome, when successfully launched would dash down forward and into the water, and sometimes (under apparently identically like conditions) would sweep almost vertically upward in the air and fall back, thus behaving in entirely opposite ways, although the circumstances of flight seemed to be the same. The cause of this class of failure was finally found in the fact that as soon as the whole was upborne by the air, the wings yielded under the pressure which supported them, and were momentarily distorted from the form designed and which they appeared to possess. "Momentarily," but enough to cause the wind to catch the top, directing the flight downward, or under them, directing it upward, and to wreck the experiment. When the cause of the difficulty was found, the cure was not easy, for it was necessary to make these great sustaining surfaces rigid so that they could not bend, and to do this without making them heavy, since weight was still the enemy; and nearly a year passed in these experiments.

Has the reader enough of this tale of disaster? If so, he may be spared the account of what went on in the same way. Launch after launch was successively made. The wings were finally, and after infinite patience and labor, made at once light enough and strong enough to do the work, and now in the long struggle the way had been fought up to the face of the final difficulty, in which nearly a year more passed for the all-important difficulty of balancing the aërodrome was no reached, where it could be discriminated from other preliminary ones which have been alluded to, and which at first obscured it. If the reader will look at the hawk or any soaring bird, he will see that as it sails through the air without flapping the wing, there are hardly two consecutive seconds of its flight in which it is not swaying a little from side to side, lifting one wing or the other, or turning in a way that suggests an acrobat on a tight rope, only that the bird uses its widely outstretched wings in place of the pole.

There is something, then, which is difficult even for the bird in this act of balancing. In fact, he is sailing so close to the wind in order to fly at all that if he dips his head but the least he will catch the wind on the top of his wing and fall, as I have seen gulls do, when they have literally tumbled toward the water before they could recover themselves.

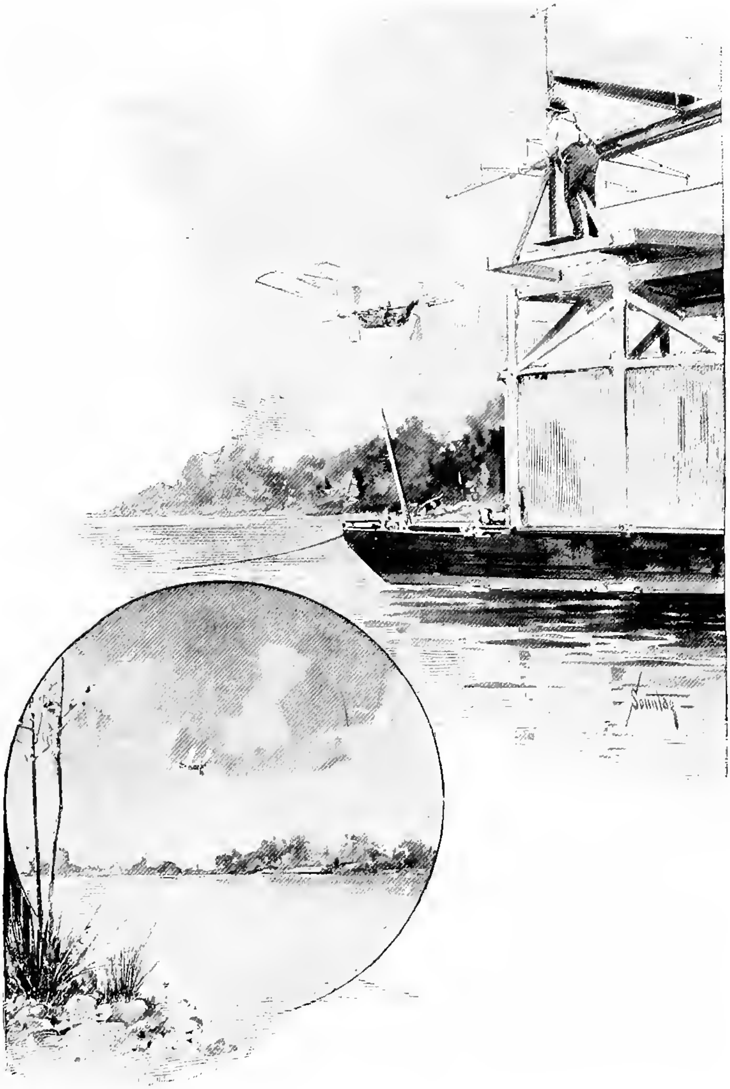
Besides this, there must be some provision for guarding against the incessant, irregular currents of the wind, for the wind as a whole—and this is a point of prime importance—is not a thing moving along all of a piece, like water in the Gulf Stream. Far from it. The wind, when we come to study it, as we have to do here, is found to be made of innumerable currents and countercurrents, which exist altogether and simultaneously in the gentlest breeze, which is in reality going fifty ways at once, although, as a whole, it may come from the east or the west; and if we could see it, it would be something like seeing the rapids below Niagara, where there is an infinite variety of motion in the parts, although there is a common movement of the stream as a whole.

All this has to be provided for in our mechanical bird, which has neither intelligence nor instinct, without which, although there be all the power of the engines requisite, all the rigidity of wing, all the requisite initial velocity, it still can not fly. This is what is meant by balancing, or the disposal of the parts, so that the air ship will have a position of equilibrium into which it tends to fall when it is disturbed,



PREPARING TO LAUNCH THE AERODROME.

From a photograph by A. Graham Bell, esq.



THE AERODROME IN FLIGHT, MAY 6, 1896.

Two views from instantaneous photographs taken by A. Graham Bell, esq.

and which will enable it to move of its own volition, as it were, in a horizontal course.

Now the reader may be prepared to look at the apparatus which finally has flown. (See diagram.) In the completed form we see two pairs of wings, each slightly curved, each attached to a long steel rod which supports them both, and from which depends the body of the machine, in which are the boilers, the engines, the machinery, and the propeller wheels, these latter being not in the position of those of an ocean steamer, but more nearly amidships. They are made sometimes of wood, sometimes of steel and canvas, and are between 3 and 4 feet in diameter.

The hull itself is formed of steel tubing. The front portion is closed by a sheathing of metal which hides from view the fire grate and apparatus for heating, but allows us to see a little of the coils of the boiler and all of the relatively large smokestack in which it ends. The conical vessel in front is an empty float, whose use is to keep the whole from sinking if it should fall in the water.

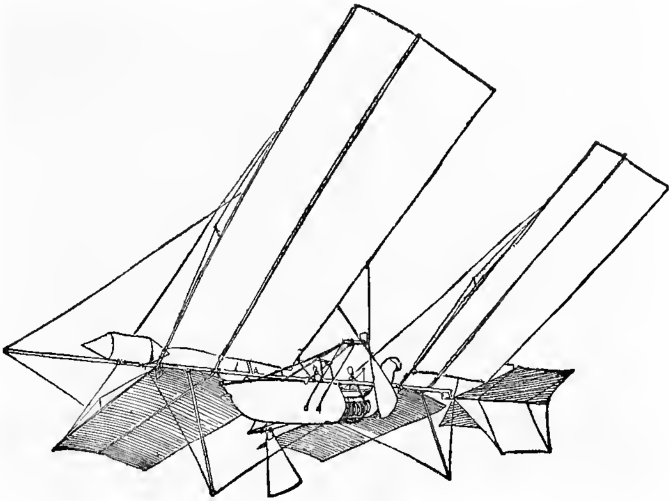


Diagram of the aërodrome.

This boiler supplies steam for an engine of between 1 and 1½ horsepower, and, with its fire grate, weighs a little over 5 pounds. This weight is exclusive of that of the engine, which weighs, with all its moving parts, but 26 ounces. Its duty is to drive the propeller wheels, which it does at rates varying from 800 to 1,200, or even more, turns a minute, the highest number being reached when the whole is speeding freely ahead.

The rudder, it will be noticed, is of a shape very unlike that of a ship, for it is adapted both for vertical and horizontal steering. It is impossible within the limits of such an article as this, however, to give an intelligible account of the manner in which it performs its automatic function. Sufficient it is to say that it does perform it.

The width of the wings from tip to tip is between 12 and 13 feet, and the length of the whole about 16 feet. The weight is nearly thirty pounds, of which about one-fourth is contained in the machinery. The engine and boilers are constructed with an almost single eye to economy of weight, not of force, and are very wasteful of steam, of which they spend their own weight in five minutes. This steam might all be recondensed and the water re-used by proper condensing apparatus, but this can not be easily introduced in so small a scale of construction. With it the time of flight might be hours instead of minutes, but without it the flight (of the present aërodrome) is limited to about five minutes, though in that time, as will be seen presently, it can go some miles; but owing to the danger of its leaving the surface of the water for that of the land, and wrecking itself on shore, the time of flight is limited designedly to less than two minutes.

I have spared the reader an account of numberless delays, from continuous accidents and from failures in attempted flights, which prevented a single entirely satisfactory one during nearly three years after a machine with power to fly had been attained. It is true that the aërodrome maintained itself in the air at many times, but some disaster had so often intervened to prevent a complete flight that the most persistent hope must at some time have yielded. On the 6th of May of last year I had journeyed, perhaps for the twentieth time, to the distant river station and recommenced the weary routine of another launch, with very moderate expectation indeed; and when on that, to me, memorable afternoon the signal was given and the aërodrome sprang into the air[1] I watched it from the shore with hardly a hope that the long series of accidents had come to a close. And yet it had, and for the first time the aërodrome swept continuously through the air like a living thing, and as second after second passed on the face of the stop watch until a minute had gone by and it still flew on, and as I heard the cheering of the few spectators, I felt that something had been accomplished at last, for never in any part of the world or in any period had any machine of man's construction sustained itself in the air before for even half of this brief time. Still the aërodrome went on in a rising course until, at the end of a minute and a half (for which time only it was provided with fuel and water), it had accomplished a little over half a mile, and now it settled rather than fell into the river with a gentle descent. It was immediately taken out and flown again with equal success, nor was there anything to indicate that it might not have flown indefinitely except for the limit put upon it.

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[1] The illustration from an instantaneous photograph by Mr. Bell, shows the machine after Mr. Reed, who was in charge of the launch (and to whom a great deal of the construction of the aërodrome is due), has released it, and when it is in the first instant of its aërial journey. (Pl. III.)

I was accompanied by my friend, Mr. Alexander Graham Bell, who not only witnessed the flight, but took the instantaneous photograph of it which has been given. He spoke of it in a communication to the Institute of France [and in a similar communication to Nature, given in full on page 199]. * * *

On November 28 I obtained, with another aerodrome of somewhat similar construction, a rather longer flight, in which it traversed about three-quarters of a mile, and descended with equal safety. In this the speed was greater, or about 30 miles an hour. The course of this date is indicated by the dotted line in the diagram. We may live to see airships a common sight, but habit has not dulled the edge of wonder, and I wish that the reader could have witnessed the actual spectacle. "It looked like a miracle," said one who saw it, and the photograph, though taken from the original, conveys but imperfectly the impression given by the flight itself.

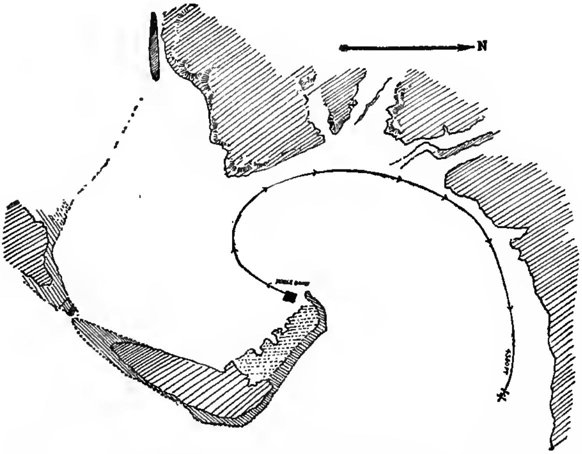
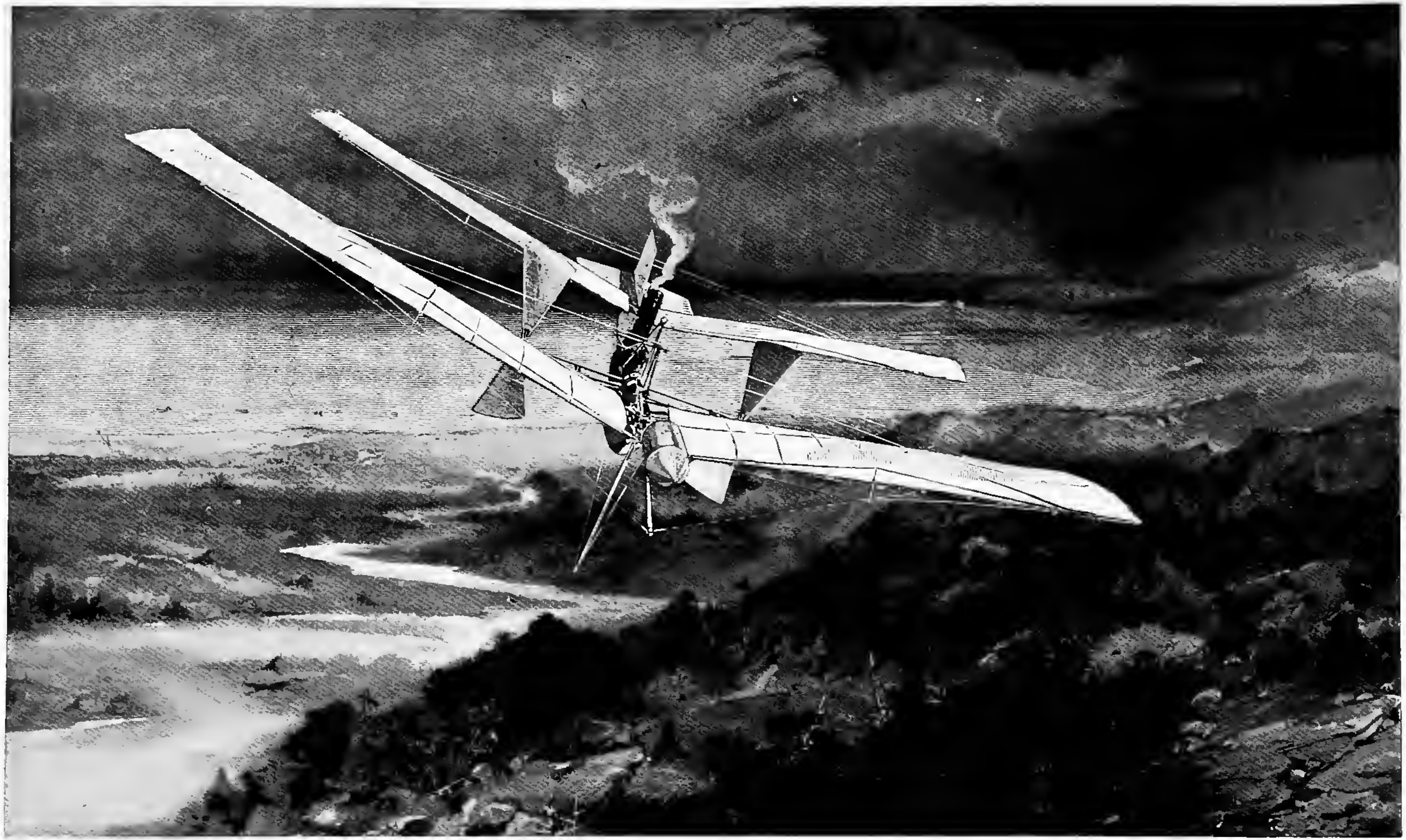


Diagram showing the course of the aërodrome in its flight on the Potomac River at Quantico.

And now, it may be asked, what has been done? This has been done: A "flying machine," so long a type for ridicule, has really flown; it has demonstrated its practicability in the only satisfactory way—by actually flying—and by doing this again and again under conditions which leave no doubt.

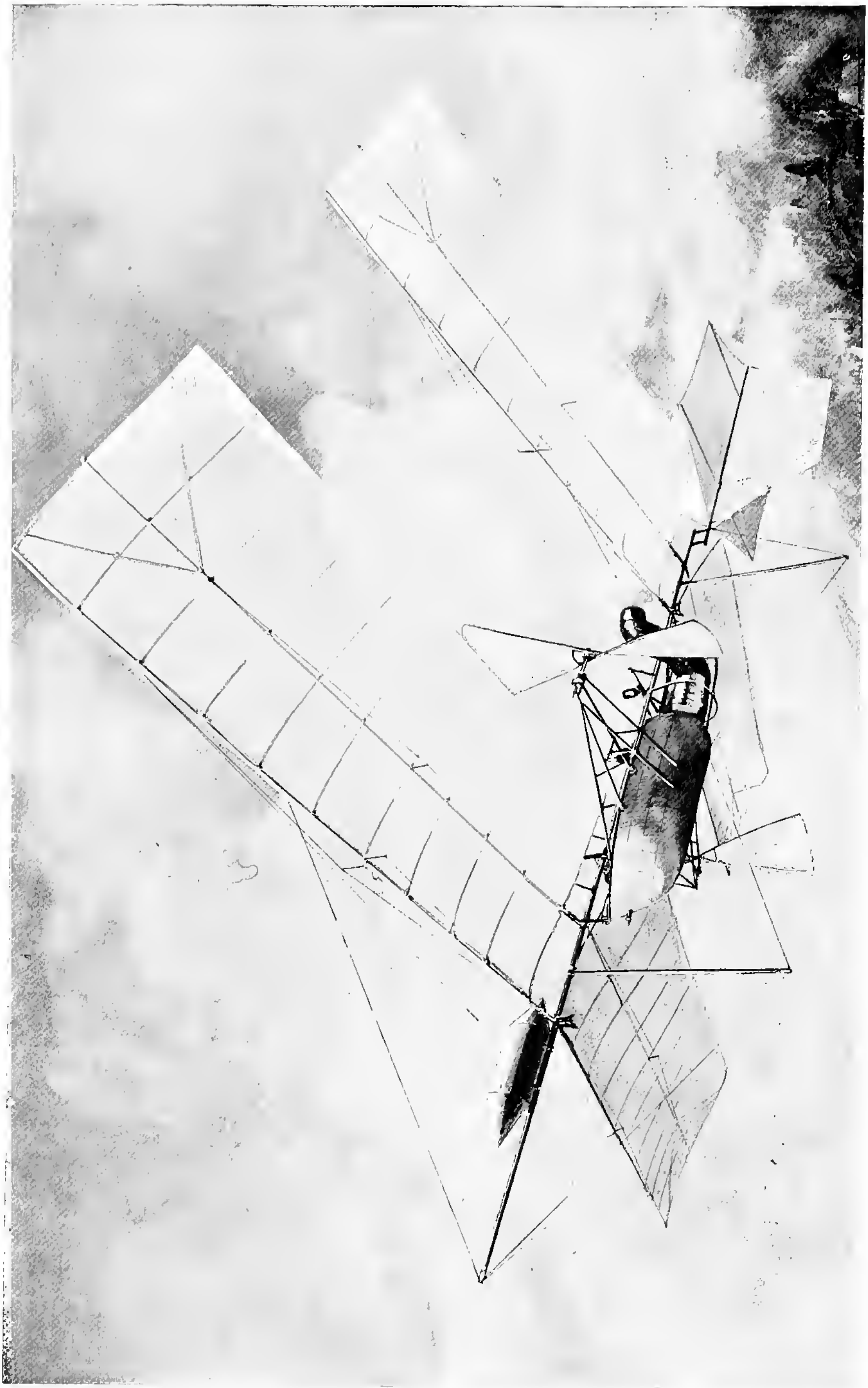
There is no room here to enter on the consideration of the construction of larger machines, or to offer the reasons for believing that they may be built to remain for days in the air, or to travel at speeds higher than any with which we are familiar. Neither is there room to enter on a consideration of their commercial value, or of those applications which will probably first come in the arts of war rather than those of peace; but we may at least see that these may be such as to change the whole conditions of warfare, when each of two opposing hosts will have its every movement known to the other, when no lines of fortification will keep out the foe, and when the difficulties of defending a country against an attacking enemy in the air will be such that we may hope that this will hasten rather than retard the coming of the day when war shall cease.

I have thus far had only a purely scientific interest in the results of these labors. Perhaps if it could have been foreseen at the outset how much labor there was to be, how much of life would be given to it, and how much care, I might have hesitated to enter upon it at all. And now reward must be looked for, if reward there be, in the knowledge that I have done the best I could in a difficult task, with results which it may be hoped will be useful to others. I have brought to a close the portion of the work which seemed to be specially mine—the demonstration of the practicability of mechanical flight—and for the next stage, which is the commercial and practical development of the idea, it is probable that the world may look to others. The world, indeed, will be supine if it do not realize that a new possibility has come to it, and that the great universal highway overhead is now soon to be opened.



MR. LANGLEY'S AERODROME IN FLIGHT.

A view from above.



THE AERODROME IN FLIGHT.

A view from below.

Missing Page

— 1358 —

THE GREATEST FLYING CREATURE.

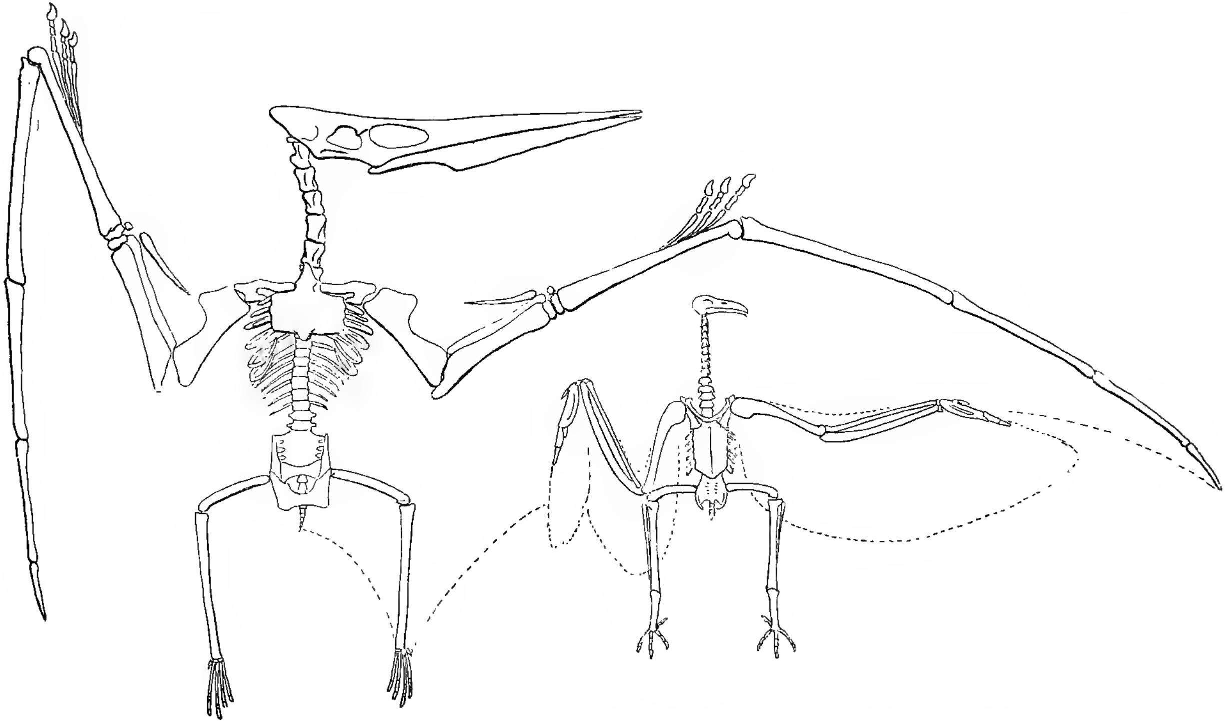
BY

S. P. LANGLEY,

INTRODUCING A PAPER ON THE PTERODACTYL ORNITHOSTOMA
BY F. A. LUCAS.

FROM THE SMITHSONIAN REPORT FOR 1901, PAGES 649-659
(WITH PLATES I-VII).

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1902.



SKELETON OF THE GREAT PTERODACTYL ORNITHOSTOMA COMPARED WITH THAT OF THE CONDOR.

The Pterodactyl after a figure by Prof. S. W. Williston.

THE GREATEST FLYING CREATURE.

By S. P. Langley.

(Introducing a paper by F. A. Lucas.)

A question of interest to all who are attracted to the subject of aerial navigation by flying machines (or things heavier than the air, and which, therefore, do not float like a balloon, but are dependent entirely on some mechanical power for their support) is, "What has nature herself done in the way of large flying machines, and are the birds which we see now the limit of her ability to construct them?"

In former epochs of our planet's history there were larger flying creatures than now, notably the Pterodactyl, "a brother to dragons," a reptile rather than a bird, but a reptile with enormously great wings. We do not know just how great this was in the living creature, except conjecturally, for we have only the skeleton. To take the expanse of the wing skeleton of a bird as giving us the expanse of wing of the actual bird would be to greatly underestimate it, the stretch of the skeleton being much less. The skeleton (which is all we have left of the Pterodactyl, a featherless reptile, and in that important respect different from a bird) will be more nearly in expanse that of the living creature.

We have here in the illustration (Pl. I) a larger than ordinary specimen of Ornithostoma, a Pterodactyl whose skeleton indicates a spread of wing of about twenty feet.

It is compared with that of the condor, nearly the largest bird now on the planet.

For my immediate purpose I will recall to the reader that birds are divisible into two classes: (1) those who soar with little motion of their wings, and yet in some mysterious manner keep their generally weighty bodies afloat on the yielding air, and (2) those who flap their wings.

Ornithostoma belongs almost unquestionably to the first of these classes. Its weight is not to be exactly estimated, but from a variety of considerations, part of which are quoted by Mr. Lucas in the ensuing paper, it is possible that the average specimen of Ornithostoma, in spite of its great wing space, did not weigh over thirty pounds.

Now we wish for our especial purpose of comparing this bird with other flying things, to know (*a*) the supporting area in square feet, (*b*) the weight, and (*c*) the power for (1) a flying machine of man's invention, which has actually flown for comparatively long distances, (2) like facts for this the largest of nature's flying machines, and (3) for some of our present birds. To recapitulate, we need for our special purpose at least the following data for any flying thing, namely, (1) the supporting area in square feet, (2) the weight in pounds, and (3) the horsepower which drives it through the air.

It is evidently impossible to exactly recover all of these for the Pterodactyl, and hard to definitely establish all three even in living specimens, but we may assume in the case of the horsepower that it is proportioned to the area of the attachment of the muscles which moved the bird in flight, an assumption which is doubtless only approximately true, but may serve our immediate purpose. With this understanding I present, together with an instantaneous photograph of a steel flying machine in actual flight (Pl. II) (repeated here from a previous publication), a diagram (Pls. III, IV) representing the above three facts in the case of (1) the flying machine, (2) the Pterodactyl (Ornithostoma), (3) the condor, and (4) the buzzard, all soaring things, and (5) the wild goose, (6) the pigeon, and (7) the humming bird, which last three fly by moving their wings.

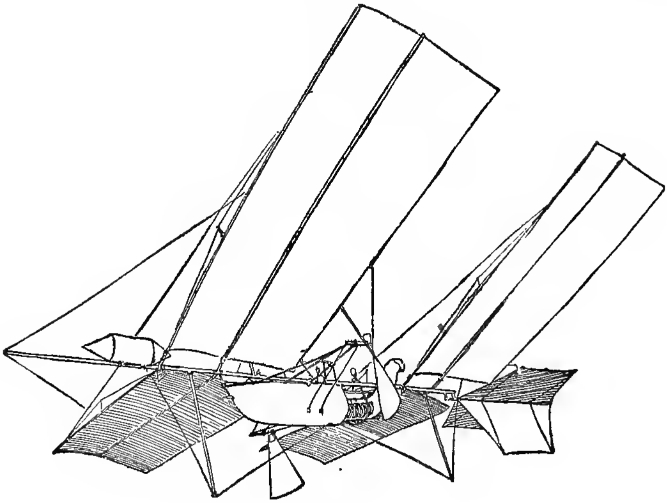
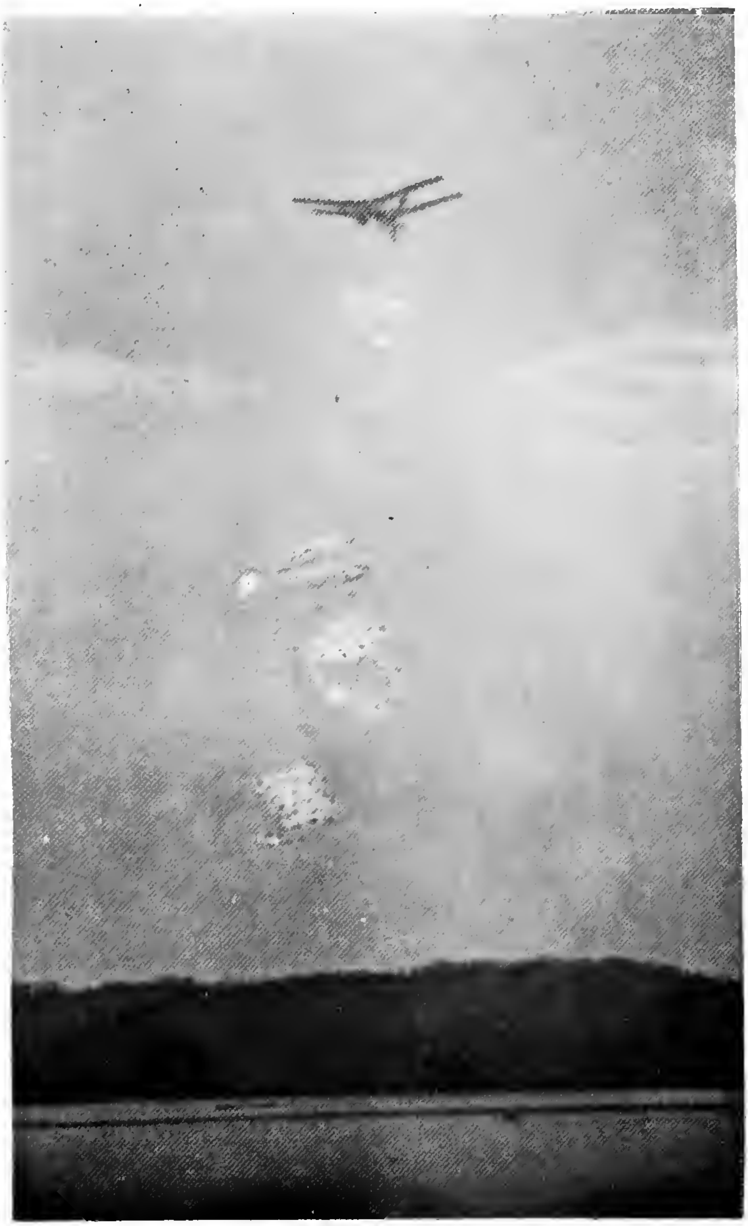


Diagram of the Aërodrome.

This steel flying machine shown in the instantaneous photograph had a supporting area of 54 square feet, a weight of 30 pounds, developed 1½ horsepower, and repeatedly flew from one-half a mile to three-quarters of a mile. These facts are represented in the diagram by the three rectangular figures whose areas are proportional to these values. Immediately after it comes nature's greatest flying machine,



LANGLEY'S AERODROME NO. 5 IN FLIGHT, MAY 6, 1896.

From instantaneous photograph by A. Graham Bell, esq.

the Pterodactyl. This may have been quite 20 feet from tip to tip of wing. The paleontologist says that approximately the wing surface was 25 square feet, the weight something like 30 pounds, and I infer from the consideration just quoted that the power was probably less than 0.05 horsepower; the immensely greater economy and efficiency of nature in the respect of power being most strikingly shown by the size of the small rectangle as compared with that in the flying machine of man's invention.

After this comes the condor, preeminently a soarer. Its stretch of wing is 9 to 10 feet, its supporting area very nearly 10 square feet, its weight 17 pounds, and the approximate horsepower it develops (inferred from the facts already stated) scarcely 0.05.

Next comes the turkey buzzard, whose stretch of wing is 6 feet, its supporting area a little over 5 square feet, its weight 5 pounds, and the approximate horsepower it develops (as above) 0.015.

All the above are soaring birds. I now pass to another order of birds, which flap their wings. The wild goose, with a supporting area of 2.7 square feet, has a weight of 9 pounds, and needs a proportionately greater power of nearly 0.026 horsepower to drive it, as against scarcely 0.02 horsepower in the last example.

Next we have another familiar bird, the pigeon, which drives itself by flapping the wings. This has an area of about 0.7 of 1 square foot, a weight of 1 pound, and a horsepower of 0.012.

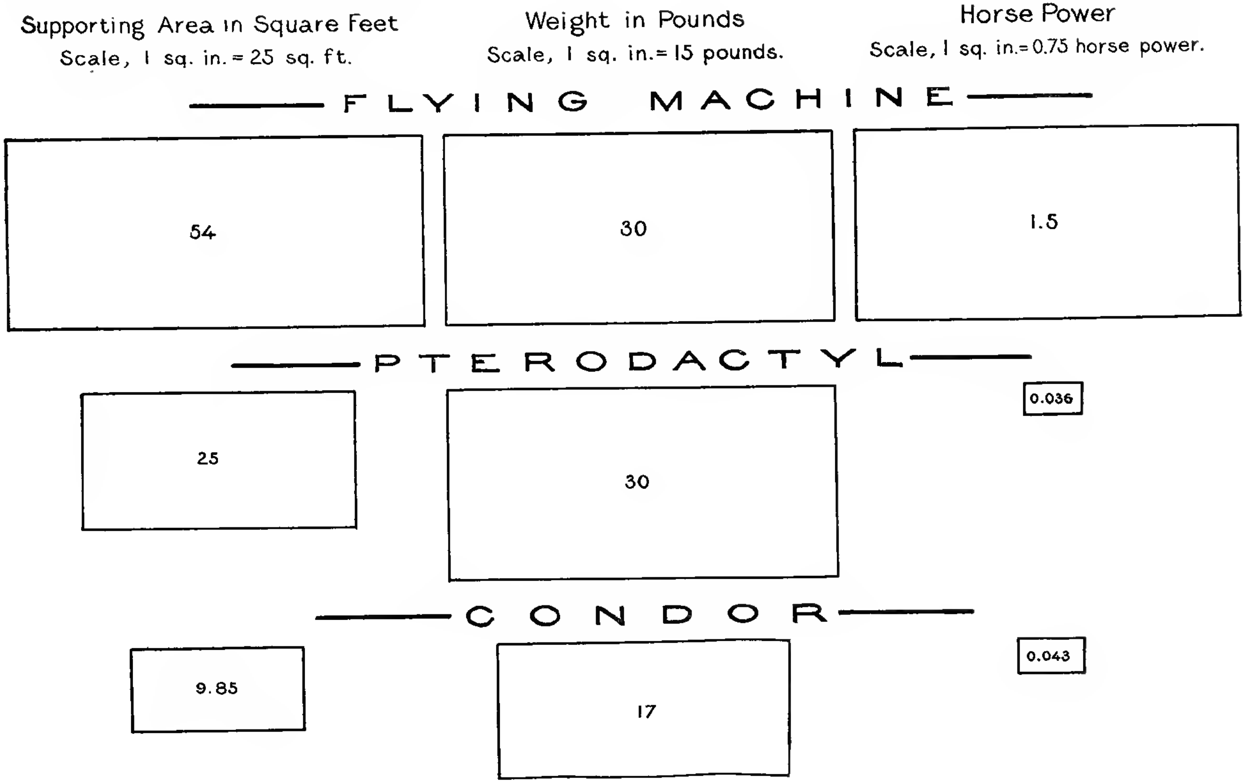
Below this we come to the humming bird, whose area, being shown on the same scale as the others, is almost too small to be distinguished on the page, but which has a supporting surface of nearly 0.03 of a square foot, a weight less than 0.02 of a pound, and a horsepower of probably not over 0.001. (All these values, as we have already said, are but approximative.)

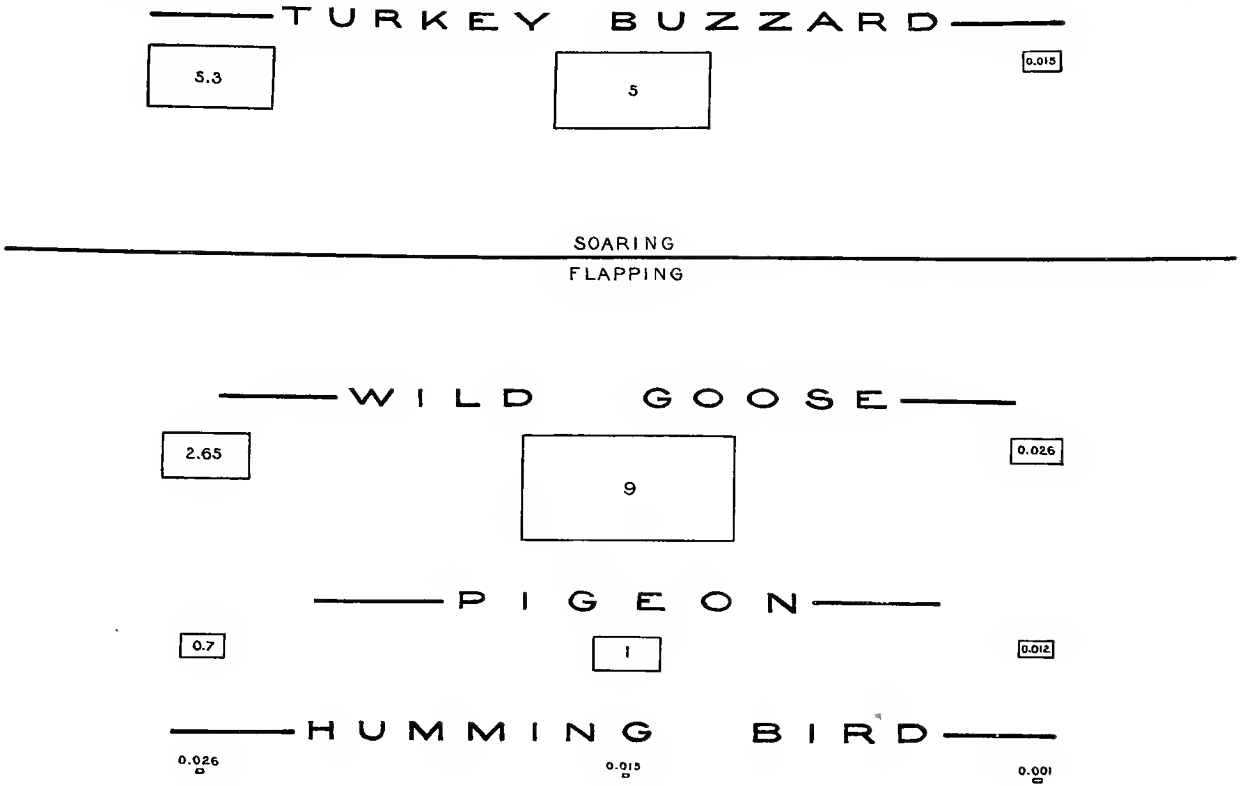
Particular attention is to be paid to the fact that regarding the ratios of supporting surface to weight supported, these ratios are not only not the same in all the birds, but themselves differ greatly, but systematically, with the absolute weight. If we inquire how much 1 horsepower would support, for instance, supposing the ratios of sustaining surface (i. e., wing area) to weight to be constant, we find that 1 horsepower would, in the flying machine, support 20 pounds with 36 square feet area of wing (i. e., 1¾ square feet to a pound); and that, passing to the flapping birds, if the wild goose were to preserve the same relations on an enlarged scale, its 1 horsepower would support 346 pounds of weight with the use of 101 square feet of wing surface or 0.29 square feet to the pound; that in the pigeon 1 horsepower would support 83 pounds of weight with the use of 58 square feet of wing surface or 0.7 square feet to the pound, and that in the humming bird 1 horsepower would support 15 pounds of weight with the use of 26 square feet of wing surface or 1.73 square feet to the pound. So that, broadly speaking, so far as these few examples go, the larger the creature the less relative surface and power is needed for its support.

From the obvious mathematical law that the area in bodies in general increases as the square of their dimensions, while their weight increases as the cube, it is an apparently plain inference that the larger the creature or machine the less the relative area of support may be (that is, if we consider the mathematical relationship, without reference to the question whether this diminished support is actually physically sufficient or not), so that we soon reach a condition where we can not imagine flight possible. Thus, if in a soaring bird which we may suppose to weigh 2 pounds we should find that it had 2 square feet of surface, or a ratio of a foot to a pound, it would follow from the law just stated that in a soaring bird of twice the dimension we should have a weight of 16 pounds and an area of 8 square feet, or only half a square foot of supporting area to the pound of weight, so that if flight is possible in the first case it would appear to be highly improbable in the second. The difficulty grows greater as we increase the size, for when we have a creature of three times the dimensions we shall have twenty-seven times the weight and only nine times the sustaining surface, which is but one-third of a foot to a pound. This is a consequence of a mathematical law, from which it would appear to follow that we can not have a flying creature much greater than a limit of area like the condor, unless endued with extraordinary strength of wing.

But this apparently necessary mathematical consequence is not the law of nature, for while it is found that in the larger bird a smaller area for each pound of the weight is given under the law than in the smaller bird, it is also found (what is another thing) that this smaller aréa is nevertheless sufficient, and that from the mathematical law just cited there does not follow the apparently obvious consequence (notably in the larger creatures like the condor, perhaps less notably in such a creature as the Pterodactyl) that the bird can not be supported, and while the fact is certain that it can, the cause of this does not seem to be clearly known.

Special cases, it may be said, may furnish an exception to what in the nature of things must be the general rule. Such, however, again does not seem to be the fact. This anomaly which is even now not generally appreciated seems to have been first noticed by a French observer, M. de Lucy, who about 1868 published a memoir, which I have not seen in the original, but an English translation of which was published in the Fourth Annual Report of the Aeronautical Society of Great Britain for 1869, and an extract from which is here reproduced. The same facts are given at greater length in an article by Dr. Karl Müllenhoff, of Berlin, in the Archiv für die Gesammte Physiologie, Volume XXXV from which Plate V is taken.


Supporting Area in Square Feet
Scale, 1 sq. in. = 25 sq. ft.
Weight in Pounds
Scale, 1 sq. in. = 15 pounds.
Horse Power
Scale, 1 sq. in. = 0.75 horse power.
FLYING MACHINE
54
30
1.5
PTERODACTYL
25
30
0.036
CONDOR
9.85
17
0.043


TURKEY BUZZARD
5.3
5
0.015
SOARING
FLAPPING
WILD GOOSE
2.65
9
0.026
PIGEON
0.7
1
0.012
HUMMING BIRD
0.026
0.015
0.001

M. de Lucy's table.

[From the Fourth Annual Report of the Aeronautical Society of Great Britain for 1869, page 63.]

INSECTS.

Names.	Square feet of wing surface per pound of weight.
Gnat	49
Dragon fly (small)	30
Coccinella (ladybird)	26.6
Dragon fly (common)	21.6
Tipula, or daddy longlegs	14.5
Bee	5.25
Meat fly	5.6
Drone (blue)	5.08
Cockchafer	5.15
Lucanus cervus, stag beetle (female)	4.66
Lucanus cervus, stag beetle (male)	3.75
Rhinoceros beetle	3.14

BIRDS.

Names.	Square feet of wing surface per pound of weight.
Swallow	4.82
Sparrow	2.72
Turtle dove	2.13
Pigeon	1.25

In this table each creature is supposed to be magnified or diminished in all its existing proportions till it weighs 1 pound. The surface dimensions of its wings will then be as given.

The above insects and birds vibrate their wings and do not soar. The table shows that the law (i. e., the law that the larger the creature the less the necessary relative area of support to a given weight) holds not only in the case of the large soaring bird, but in the case of smaller ones which flap their wings, and even in the case of insects. The explanation may be very near at hand, but it is not to me evident.

The accompanying table, from Mouillard's L'Empire de L'Air, deals with the same facts, and exhibits the paradoxical law that the greater the creature the smaller the (relative) supporting surface:

Table showing weight, wing area, and square feet of wing surface which sustains 1 pound of weight. [a]

Latin name.	Common name.	Weight in pounds.	Wing surface in square feet.	Square feet of wing surface per pound of weight.
Scops zorca	Screech owl	0.33	0.776	2.35
Accipiter nisus	Sparrow hawk	.336	.69	2.05
Larus melanocephalus	Black-headed gull	— .619	.92	1.49
Astur palumbarius	Gosshawk	— .641	.84	1.31
Otus brachyotus	Short-eared owl	— .67	1.50	2.24
Ibis falcinellus	Glossy ibis	— .806	1.24	1.54
Corvus corax	Raven	— 1.34	2.50	1.87
Milvus ægyptiacus	Kite	— 1.41	3.02	2.14
Pandion haliaetus	Fishhawk	— 2.80	3.01	1.08
Neophron percnopterus	Scavenger vulture	— 3.83	3.65	.95
Cathartes aura	Turkey buzzard	— 5.6	5.33	.95
Pelecanus onocrotalus	White pelican	— 6.66	6.32	.95
Phœnicopterus antiquorum	Flamingo	— 6.34	3.50	.55
Gyps fulvus	Griffon vulture	—16.52	11.38	.68
Sarcorhamphus gryphus	Condor	—16.52	9.80	.59
Otogyps auricularis	Eared vulture	—17.76	11.99	.68

[a] Data compiled chiefly from Mouillard, L. P., L'Empire de L'Air, Paris, 1881.

The curve (Plate V) shows the same facts in a graphic form, and they seem to me to deserve a fuller explanation than has yet been given to them.

I now invite the reader's attention to Mr. Lucas's interesting paper.

S. P. LANGLEY.

THE GREATEST FLYING CREATURE, THE GREAT PTERODACTYL ORNITHOSTOMA.

By F. A. LUCAS,
United States National Museum.

No one animal combines all the best features of weight, power, and wing area needed in a flying machine, for those with the greatest expanse of wing are by no means the heaviest and strongest, while the most powerful birds are not those of the longest sustained flight or those which fly to the best advantage if considered from an economical standpoint. The Frigate Bird, which is perhaps the bird of all others most at home in the air, lacks carrying capacity, being so far as mere muscle goes comparatively weak, sailing by skill and not by strength. Birds of prey, on the other hand, which can carry away a quarry of very nearly their own weight, fly when they do this by labored strokes of their powerful pinions, with an apparent expenditure of considerable power, sailing or soaring only when not encumbered by extra weight.

The Albatross, which has a maximum weight of 18 pounds and a spread of wing of 11 feet 6 inches, is the most notable example we have of long sustained flight in a heavy bird,[a] and it is the more remarkable from the fact that as the wing is extremely narrow its area is very small, not exceeding 7 square feet. The surplus lifting power of this bird is quite small, since the wing muscles on whose area we must base our estimate of the amount of force exercised in flight are comparatively small. Both the Albatross and Frigate Bird, however, are of double interest from the very fact of their great extent of wing and small amount of muscle, since they thus throw some light on the question of the length of wing that may be manipulated with a given force.

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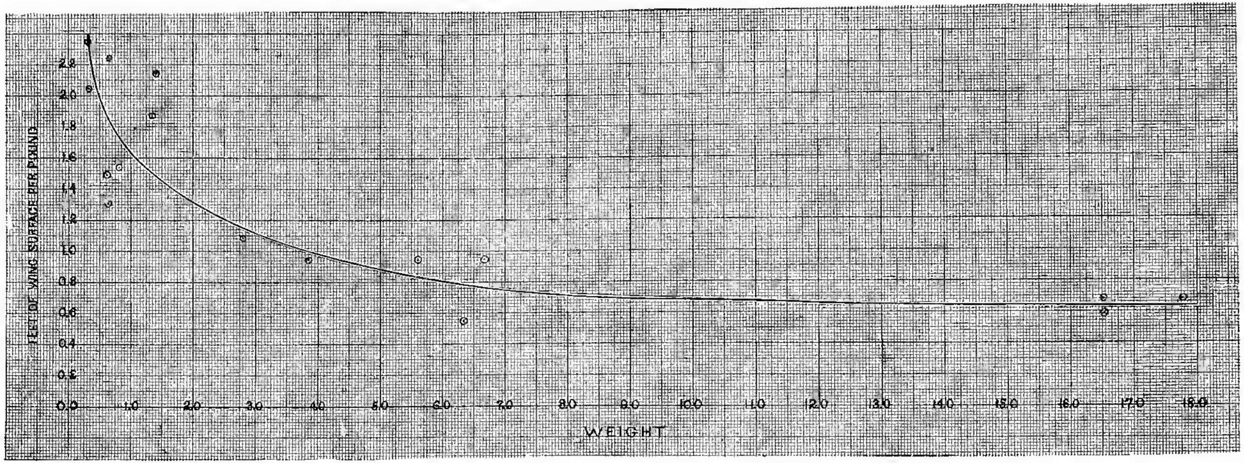
[a] Sailors sometimes catch an Albatross, fasten to it a tag bearing the name of the ship, date of capture, latitude and longitude, and then release the bird. A specimen thus tagged and subsequently taken by another ship is preserved in the museum of Brown University, showing that in twelve days it had traversed a distance of at least 3,150 miles, probably more, since the Albatross rarely flies in a direct line.

PLATE V.



CURVE SHOWING RELATIVE DECREASE OF WING SURFACE WITH INCREASED WEIGHT OF BIRD.

The Condor, and his cousin, the California Vulture, weigh about the same as an Albatross,[a] but the broad, rounded shape of their wings gives them a much greater area, and this difference is, in turn, related to differences in flight, for the great vultures soar high in the air, while the Albatross skims the sea, rarely rising to an elevation of 150 feet.

It is to be noted, however, that the question of food has something to do with the mode of flight, since the one bird seeks its food from the surface of the water, while the other mounts aloft to scan the earth in search of something eatable.

Humboldt is credited with having seen a Condor soaring above the summit of Chimborazo; but that this or any bird ever attains such an altitude[b] is more than questionable, and Whymper, the most recent and most careful observer, puts the range of the great Vulture at from 7,000 to 15,000 feet.

The Condor is said to attain a spread of wing of 15 feet, but no bird of anything like this size is preserved in any collection, and even 10 feet 6 inches from tip to tip may be looked upon as exceeding the normal or average size.[c] As the Albatross averages 10 feet from tip to tip, and is said by good observers to reach 12 or even 14 feet,[d] it may be pretty safely set down as having the greatest stretch of wing of any animal now living. Certainly the Albatross stands first in length of wing bones, for these measure 8 feet 3 inches in the great wandering Albatross, while the bones of a large Condor have a combined length of but 6 feet 1 inch. Moreover, the Albatross inhabits the wind-swept seas of the Southern Hemisphere, one of the stormiest regions of the globe, and is continually called upon to wield its pinions in the teeth of gales, and the successful manner in which this is done calls forth the admiration of the observer.

So far as carrying weight is concerned, the Trumpeter Swan stands at or near the head of the list, for this bird attains a weight of 28 pounds, and carries this far and fast with a spread of wing of 8 feet. Its mode of flight is entirely different from that of the Albatross, being performed by powerful wing beats, while the latter bird rarely flaps its wings, but sails over the water with little apparent expenditure of muscular power. In default of these birds the Wild Goose (*Bernida canadensis*) and Turkey Buzzard may serve as representatives of differences in method and apparatus of flight.

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[a] A California Vulture, 1 year old, in the National Zoological Park, weighed 18½ pounds.

[b] Birds are known to migrate at a very considerable elevation, but it is believed that none have as yet been recorded so high as 4 miles. The height of Chimborazo is 20,494 feet.

[c] A fine Condor from Patagonia had a spread of only 8 feet 8 inches, and the California Condor in the National Zoological Park at Washington measures but 9 feet 2½ inches across the wings. Like most large animals, Condors shrink wofully before a tape line.

[d] The largest of four Albatrosses measured by the writer had a spread of wings of only 10 feet 3 inches, but these were birds of 1 year and 2 years old, and many of the old birds seen were certainly much larger. The ship's carpenter claimed to have measured a bird of 12 feet spread.

The goose, like his relative the swan, flies by means of the strokes of his wings and carries a weight of 9 pounds, with a wing area of 2.65 square feet and a muscle area of 8.84 square inches; the sailing buzzard, with a weight of 5 pounds, has a wing area of 5.3 square feet and a muscle area of 5.12 square inches. Thus the one bird has 0.3 square foot area of wing per pound of weight, while the other has 1.06 square feet per pound of weight. Or, if we wish to compare the area of wing to the area of sternum, we may say that in the goose this ratio is 43 to 1 and in the buzzard 149 to 1. The minimum of wing area, both positively and comparatively, is reached in the humming birds, which may be typified by a species common in Barbados (*Eulampis chlorolæmus*). This little bird, weighing 0.015 pound, has a wing area of 0.026 square foot, and a muscle area of 0.33 square inch, a ratio of 11.4 to 1, while, if brought up to ounces, the wing area per ounce would be but 0.76 square inch.

These differences are dwelt on at some length in the introduction to this paper, where they are graphically expressed by means of diagrams and compared with the weight, horsepower, and supporting area of a flying machine.

The buzzard may be compared to a racing yacht with small hull and great spread of canvas; the humming bird, like a torpedo boat, is mainly engine.

Mammals may be practically left out of consideration in discussing large flying creatures, for while many of the bats fly with the utmost dexterity, none of them attain any considerable size, the largest of the fruit bats (*Pteropus edulis*) weighing under 3 pounds and having a spread of wing of 5 feet. Almost everyone is acquainted with the rapid fluttering flight of small bats, and it need only be said that the large species fly with measured wing beats not unlike those of a crow.

Such are some of the flying forms of to-day, and, with few exceptions, they seem not to have been exceeded by any creatures of the past. *Harpagornis*, the extinct eagle of New Zealand, was larger and more powerful than any existing bird of prey, although the South American harpy eagle is a near second;[a] but the more notable exceptions were the great flying reptiles, or pterodactyls, which abounded on the shores of the inland sea that during Cretaceous time extended from the Gulf of Mexico up the Mississippi Valley and northwesterly through Kansas. And as the huge dinosaurs were the largest creatures that ever walked,

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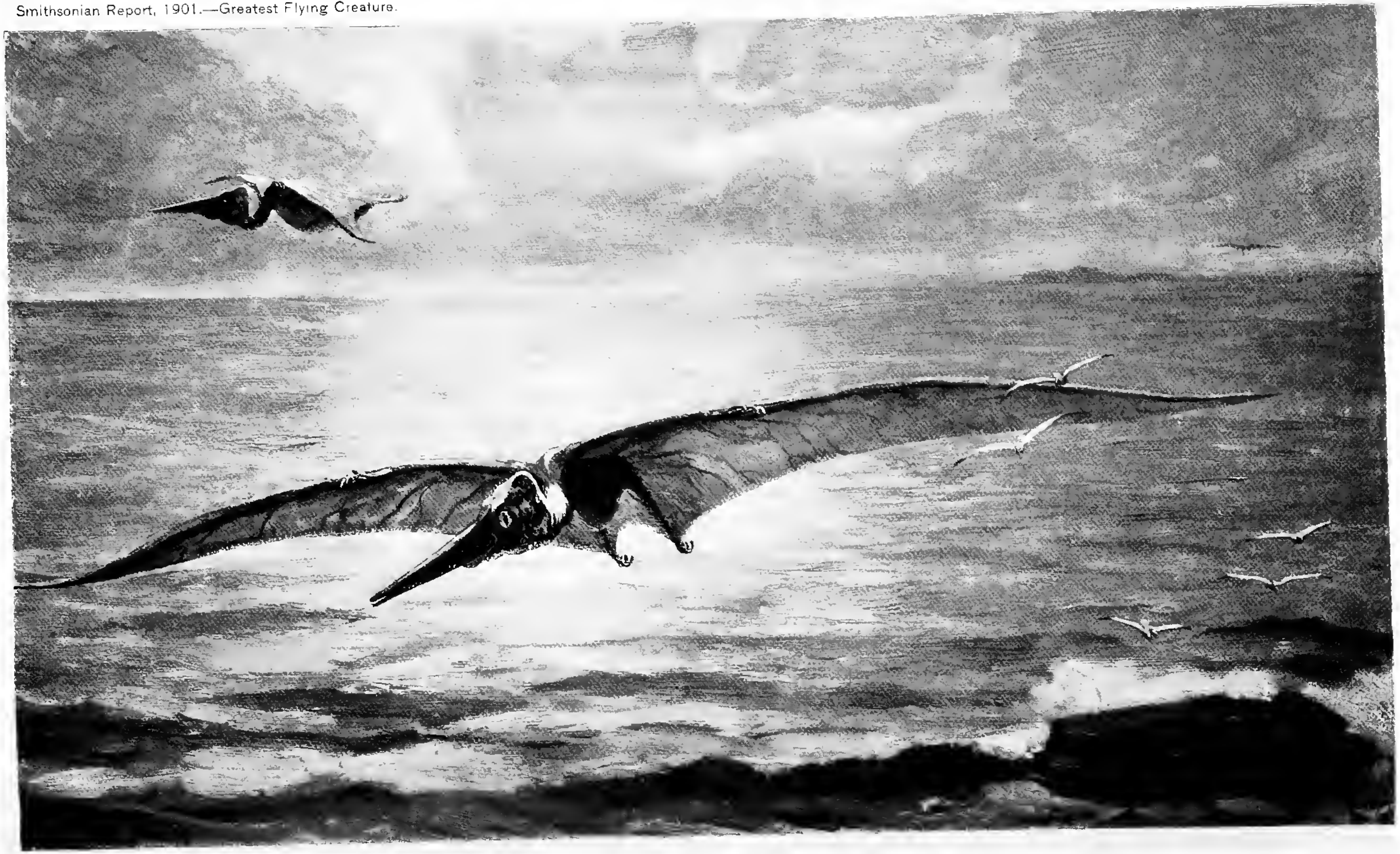
[a] A specimen of this bird, *Thrasaëtus harpyia*, in the National Zoological Park, weighs 19½ pounds.

PLATE VI.



THE GREAT PTERODACTYL ORNITHOSTOMA.

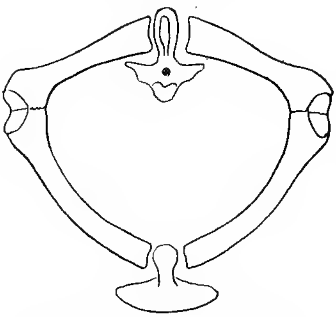
so the greatest of these pterodactyls were the largest creatures that ever flew, their outstretched wings having a spread of 20 feet from tip to tip.

There is one possible rival, a bird supposed to be a relative of the pelicans, described by Professor Cope under the name of *Cyphornis;* but as this bird is known from a small fragment only and its wing area very far from certain, *Cyphornis* may be ruled out of competition.

The greatest of the pterodactyls, *Ornithostoma ingens* (Pl. VI), has been described at some length by Prof. S. W. Williston, of the State University of Kansas, and from his articles have been taken the facts relating to this curious creature that are herein embodied.

The great moa marks one extreme of specialization, the disproportionate size of the hind as compared with the fore limbs, for this big bird had legs 6 feet long and no fore legs at all; *Ornithostoma* marks the other extreme with a wing 9 feet in length and a leg so small and weak as to be of little use save for spreading the wing membrane. For, like other pterodactyls, whose wings are accurately known from their impressions in the fine-grained lithographic stone of Solenhofen, this species doubtless had a membranous wing something like that of a bat. As for the body, being that of a reptile, it must have been naked and possibly covered with small scales like those on the body of an iguana, so that on a small picture the skin would appear quite smooth. While the body was small in comparison with the extent of wing, the head, which was principally beak, was very nearly 4 feet long, extending backward to form a large but thin crest. This has a direct relation to the enormous length of the beak, since it furnished a point of attachment for muscles whose pull counterbalanced the leverage of the front part of the head. The beak was dagger-like, being very narrow, pointed, and quite toothless. Whether this beak was covered with a thin, hard skin, like the epidermis on the head of a crocodile, or with horn, like the bill of a bird, is not positively determined, but the weight of evidence is in favor of the former, since none of the pterodactyls yet found show any traces of a horny bill. In the peculiar shape of the lower, back portion of the beak there is a suggestion of the former presence of a small pouch, like that found in cormorants, and this would be in accord with the supposed fish-eating habits of *Ornithostoma.* Like other animals with long, narrow wings, *Ornithostoma* doubtless sailed somewhat after the manner of the albatross. This is inferred not only from the size and shape of the wing, but from the comparatively small size of the breastbone, to which were attached the muscles used in flight. Birds which fly by strokes of their pinions have the breastbone deeply keeled to furnish room for the attachment of the wing muscles, and the size of this keel is in direct relation to the rapidity of the wing strokes, reaching its maximum in the humming birds, in which the wings are vibrated so rapidly as to be invisible. Birds which sail have the breast muscles much reduced, and the extreme of reduction is found in the frigate bird, which, with a spread of wing of 6 feet 4 inches, has a muscular area of only 3.50 square inches.[a]

There is another point in the anatomy of *Ornithostoma* besides length of pinion that lends strength to the supposition that it sailed, and this is found in the structure of the fore limb. It was pointed out by Mr. Huffaker that in spite of the deficiency of muscle shown by soaring birds the support of the wing was very strongly built; thus the frigate bird with its small breastbone has the bones of the shoulder joint firmly united with one another and with the breastbone. In the albatross strength is gained by shortening and widening the bone to which the wing is directly fastened and giving it a broad base for attachment to the breastbone. In the great pterodactyls strength was obtained by bracing the shoulder blade against the backbone, in the manner shown in the diagram; thus the body, so to speak, was slung from the wings. In addition, three sections of the backbone were united in one piece in order to give a firm point of attachment, the whole arrangement curiously suggesting the fore leg of a turtle.



How the wing of *Ornithostoma* is supported.

In spite of its great extent of wing, *Ornithostoma* was not a heavy animal, possibly not so heavy as the trumpeter swan, for the body was small and the bones reached the extreme of lightness, being far lighter than in any bird. This may be appreciated by quoting Professor Williston's remark that the bones were almost papery in their character, one of the finger bones 26 inches long and 2 inches in diameter being no thicker than a cylinder of blotting paper. The same authority, basing his estimate on this extreme lightness of structure and the small size of the body, places the weight of one of these pterodactyls at only 25 pounds, and with this weight and its great spread of wings the creature must have flown as lightly as a butterfly. Even if we increase the estimated weight by 20 per cent, we have a creature weighing but 30 pounds, so that the body was even more an appendage to the wings than in the frigate bird, and seems to have been just heavy enough to counterbalance the weight of head and neck and insure equilibrium.

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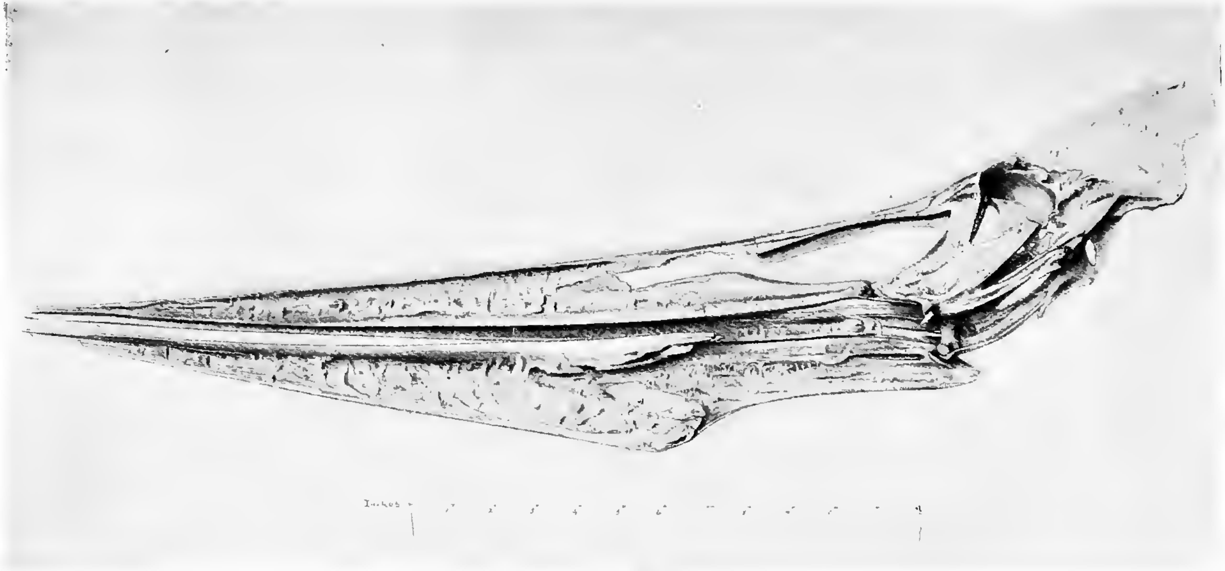
[a] This is stated with some hesitancy, as no sternum of a large albatross is available, and it may be that, all things considered, the albatross has the least amount of wing muscle. The ratio of wing muscle to wing is smaller in the turkey buzzard than in the frigate bird, being, respectively, 1:125 and 1:114, this owing to the much broader wing of the buzzard. On the other hand, the great humming bird (*Patagona gigas*) has a ratio of muscle to wing area of 1:23, and a small species a ratio of but 1:11.39.



SKULL OF THE GREAT PTERODACTYL ORNITHOSTOMA.

From a specimen in the Yale University Museum.

As *Ornithostoma* was capable of long sustained flight, and as its bones are found under conditions indicating that it went far out to sea, it is not improbable that it fed largely or entirely on fish. That they formed a part of its diet is certain, for fish bones and scales are found with the remains of pterodactyls, and it is easy to imagine this great reptile gliding over the sea, with outspread wings, snatching up fish right and left with its long beak as easily as a museum assistant picks them out of a jar of alcohol with a pair of forceps. The bird in the foreground is represented in our illustration as just turning to its right, the left wing being advanced and raised to cause the turn.

With its small body and enormous wings *Ornithostoma* may be looked upon as the king of flying creatures, and as more highly specialized than any flying animal before or since his time.

Finally, it is an interesting question as to whether or not the condor, the albatross, and the pterodactyl mark the limit of size attainable by flying creatures—are the mechanical difficulties in the way of using wings so great that evolution stops at a weight of 30 pounds and a spread of wing of 20 feet? Would animals above that size have trouble in manipulating their wings and be unable to compete with smaller and more active forms, or is it that the exigencies of life have never called for the development of a larger creature?

These are queries that may not be settled offhand, and it may only be said that the vast majority of birds are small and agile, and that, although birds and pterodactyls flew side by side over the Cretaceous seas and shores, the birds never reached the size of their reptilian associates, and, so far as we know, these mark the limit of size among flying animals.

— 1349 —

THE LAWS OF NATURE.

BY

S. P. LANGLEY.

FROM THE SMITHSONIAN REPORT FOR 1901, PAGES 545-552.

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1902.

THE LAWS OF NATURE.[a]

By S. P. Langley.

We say that nature is unchanging, and so perhaps it is, in the eye of some eternal being, but not in ours, for the things that we see from day to day, appear permanent only by comparison to the duration of our own brief life, and our own little experience.

An inhabitant of the land where nature has just passed through such an awful convulsion, with a loss of life greater for so short a time than history has ever recorded, might have said in the morning that nature never changes, because it had never changed in his own little experience; but he would not have said so at that day's close. Now the experience of the entire human race is far briefer relative to nature's duration than that of one of these islanders, who knew the green mountain with its fresh lakes only as a place of quiet rest up to the moment when the gates of hell were opened beneath it.

Nature, then, really changes, and would apparently do so if man were not here; for it is not man's varying thoughts about nature that make her change. But there is something quite different from nature which does change because of man, and which apparently would not change if he were not here. This is what he calls the 'laws of nature.' The assumption that there are such things is due to him, and such 'laws' are known only through his mind, in which alone nature is seen.

It is perhaps an hard saying to most that there are no such things as 'laws of nature'; but this is the theme on which I have to speak.

These, then, are the laws of his own mind, or the effects of his own mind, which he projects outside of himself and imagines to be due to some permanent and unalterable cause having an independent existence; and this, not only because his season for observation is but a moment in the passage of nature's eternal year, but because with his pathetic sense of his own weakness, he would gladly stay himself on the word of some unchanging being. It is because this sense of dependence is strangely joined with such self-conceit that when he listens to what he himself says, he calls it the voice of God. From these twin causes, arising both from his inability as a creature of time to observe what is eternal, and again from his own overweening sense of his own capacity, he looks for some immutable being whom he believes to have written man's own ideas in what he calls 'the book of nature.'

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[a] A paper read before the Philosophical Society of Washington, May 10, 1902.

I am not questioning the existence of such a being as the 'Author of Nature'; but asking if such a volume as is imputed to him, ever really existed. The very phrase, 'book of nature' is a legacy from moribund mediæval notions of a lawgiver; and it, with the vitality of words which carry to us dying ideas, has lived on to our own time, when we can no longer believe it, although it is still upon our lips, and to convince ourselves of this we need only pause a moment to ask the simple question whether there is any authority who has prepared a clearly written book of statutes, in which we can really read nature's laws.

The question answers itself.

I repeat that I am not denying here the existence of such a being as the imputed author of these laws, but say that, ignorant as we are of what is being done by him, we cannot read his thoughts in our momentary vision of what is forever passing.

"For my thoughts are not your thoughts, neither are your ways my ways, saith the Lord," is a caution which, whether believers or not, would not harm us to consider; and when we say that these 'thoughts' are written in 'the book of nature,' this cannot mean that they are legible there as in a statute book where he who runs may read. If nature is to be compared to a book at all, it is to a book in the hands of infants to whom it conveys little meaning, for such are we; or rather it is like a 'book of celestial hieroglyphs, of which even prophets are happy that they can read here a line and there a line.'

I hope what I am trying to say may not bear the appearance of some metaphysical refinement on common sense. It is common sense that is intended, and the 'laws of nature' that seem to me to be a metaphysical phrase.

To decorate our own guesses at nature's meaning with this name is a presumption due to our own feeble human nature, which we can forgive for demanding something more permanent than itself, but which also leads us to have such an exalted conceit of our own opinions, as to hide from ourselves that it is these very opinions which we call nature's laws.

The history of the past shows that once, most philosophers, even atheists, thus regarded the 'Laws of Nature,' not as their own interpretations of her, but as something external to themselves, as entities partaking the attributes of Deity—entities which they deified in print with capital letters—as we sometimes do still, though these 'Laws' now are shorn of 'the glories of their birth and state' which they once wore, and are not turning out to be 'substantial things.'

But are there not really things (like the fact of gravitation, for instance) external to ourselves, which would exist whether we were here or not, and which are part of the order of nature? Apparently, yes, but part of the *laws* of nature, no!

The phrase even yet exercises a wide influence, though it has seemed to me that a significant change is taking place in the leaders of common opinion with regard to the meaning that the words convey.

I presume that the greater proportion of us here are interested in science. I may indeed assume that we all are; and I want to inquire what lesson for us, as students of nature, there lies in the fact that we are no longer impressed by her 'laws' as were the scientific men of a former generation.

It is convenient to measure the distance we have passed over by the fact that one hundred and fifty years ago, one of the acutest of reasoners, David Hume, published a still celebrated argument against miracles which within my own recollection was held to be so formidable that those who were reluctant to believe in his conclusions were still unable to offer a good refutation. The immense number of attempted refutations and their contradictory character is perhaps the best testimony for this.

Hume defines a miracle as a violation of the 'laws of nature,' and his argument, concisely stated, is that there must 'be a uniform experience against every miraculous event, otherwise the event would not merit that appellation, and as a uniform experience amounts to a proof, there is here a direct and full proof from the nature of the fact against the existence of any miracle.'

Now, while his argument is logically as conclusive as ever, it to-day convinces only those who are anxious to accept its conclusion.

What is the reason for this great change?

We may ask what the laws of nature really are, and pass from what they were thought to be by Hume, to what they are beginning to be understood to be by us, without here inquiring into the intermediate steps which brought the change about.

It seems to me that the argument which was conclusive not merely to the learned, but to the common cultivated thought of Hume's time, has never been expressly refuted when its premises were admitted, (and the generation following him admitted them); and yet this compelling argument, as it once seemed, is gradually losing its force to most minds, not through counter argument, but by an insensible change of opinion in the attitude of the thinking part of our public as compared with his, a change about certain fundamental assumptions on which the argument rested, and from his own views of the universe, to those we are beginning to take.

In the first place, the immensely greater number of things we know in almost every department of science beyond those which were known one hundred and fifty years ago has had an effect which doubtless could have been anticipated, but yet which we may not have wholly expected. It is, that the more we know, the more we recognize our ignorance, and the more we have a sense of the mystery of the universe and the limitations of our knowledge.

I believe it may be said that if not to Hume, at any rate to the majority of those about him, and to his later contemporaries, there was very much less mystery in the world than we see in it, and if it were then still occasionally said that there were "things in heaven and earth not dreamt of in 'their' philosophy," these words must have struck on the self-complacent minds of his generation, as something to be tolerated as poetic license, rather than as accurate in philosophic meaning. Compared with ours, that whole century was satisfied with itself and its knowledge of the infinite, and content in its happy belief that it knew nearly everything that was really worth knowing. This 'nearly everything' which it thought it knew about the universe, it called the 'laws of nature.'

It was to this belief in the general mind, I think, that the success of Hume's argument was due.

The present generation has begun, if not to be modest or humble, to be somewhat less arrogant in the assumption of its knowledge. We are perhaps beginning to understand, not in a purely poetical sense, but in a very real one, that there may be all around us in heaven and earth things beyond measure, of which 'philosophy' not only knows nothing, but has not dreamed.

As a consequence of this, there is growing to be an unspoken, rather than clearly formulated admission, that we know little of the order of nature, and nothing at all of the 'laws' of nature.

Now if we are, at present at least, disposed to speak of an observed 'order' of nature (not carrying with it the implication of necessity denoted by 'law'), I think we have some reason to say that there is a prescience of a change in common thought about this manner, and that it is owing to this that we are coming to be where we are.

I do not know that there is a less wide belief in the gospel miracles in our day, but if it were so, the decline in the weight given Hume's argument is not due solely to that, for it may surely be said that it was not merely an argument against gospel miracles, but against all the prodigies to be found in history, sacred and profane, where he doubtless had in mind traditions of stones falling out of heaven, cures wrought by psychological agency, and the like, all 'superstitions' to the men of his day, who, if they no longer believed in a deity, were none the less shocked by the culpable existence of such vulgar beliefs in conflict with the deified 'laws of nature,' while such 'superstitions' have in our day become subjects of modest inquiry.

Let me quote from a later writer, whose point of view is singularly different from that of Hume and his contemporaries, and who in answer to the question, 'What is a miracle?' begins by reminding us that the reply will depend very much upon the intelligence of the being who answers it, or whom the miracle is wrought for.

"To my *horse*, do I not work a miracle every time I open for him an impassable turnpike?"

"'But is not a real miracle simply a violation of the laws of nature?' ask several. What are the laws of nature? 'Is it not the deepest law of nature that she be constant?' cries the illuminated class. 'Is not the machine of the universe fixed to move by unalterable rules?'

"I believe that nature, that the universe, which no one whom it so pleases can be prevented from calling a machine, does move by the most unalterable rules. And now I make the old inquiry as to what those same unalterable rules, forming the complete statute book of nature, may possibly be?

"'They stand written in our works of science,' say you; 'in the accumulated records of man's experience.' Was man with his experience present at the creation, then, to see how it all went on? Have any deepest scientific individuals yet dived down to the foundations of the universe and gauged everything there? Alas, these scientific individuals have been nowhere but where we also are; have seen some handbreadths deeper than we see into the deep that is infinite, without bottom as without shore."

"Philosophy complains that custom has hoodwinked us from the first; that we do everything by custom, even believe by it; that our very axioms, boast as we may, are oftenest simply such beliefs as we have never heard questioned. Innumerable are the illusions of custom, but of all these perhaps the cleverest is her knack of persuading us that the miraculous, by simple repetition, ceases to be miraculous!"

A lesson for us, as people who are most of us interested in science, as to how little its most fixed conclusions may be worth, may perhaps be conveyed in an example. A century and a half ago, when the new science of chemistry won its first triumphs, the fundamental discovery which was to illuminate the whole science, the settled acquisition which it seemed to have brought to us, the thing which was going to last, was 'phlogiston.'

This had everything to recommend it in universal acceptance and in what seemed to the foremost men of the time its absolute certainty.

"If any opinion," says Priestley, "in all the modern doctrine concerning air be well founded, it is certainly this, that nitrous air is highly charged with phlogiston. If I have completely ascertained anything at all relating to air, it is this."

I am trying here to say that all laws of nature are little else than man's hypotheses about nature.

Phlogiston was then to the science of a former age, in this sense a law of nature, and at least as great a generalization as the kinetic theory of gases is to us; as widely accepted, as firmly believed and as certainly known—but what has become of it now?

Can we tell, then, in advance, by any criterion, what a 'law of nature' is?

With a curious begging of the question some answer, 'Yes, for laws of nature have this distinction, that they have never been disproved.' As if one were to say, Yes, because when they *are* disproved we deny that they are laws of nature!

Those of us who are capable of being instructed or warned by the history of human thought may, then, ask what kind of a guarantee are we to have for any other 'fact' of our new knowledge? May they not—all these 'facts'—be gone like the baseless fabric of this vision, before another hundred years are passed?

The physical sciences seem to have had less change in their theories than the mighty displacements in other branches of natural knowledge, but it is a truism to say that all are changed, and it should be a truism to add that the 'laws of nature' are not to us what they were a hundred years ago.

I repeat that of the 'order' of nature we may possibly know a little; but what are these 'laws' of nature? What celestial act of congress fixed them? In what statute book do we read them? What guarantees them? Our mistake is in believing that there is any such thing, apart from our own fallible judgment, for the thing which the 'laws of nature' most absolutely forbid one generation to believe, if it only actually happens, is accepted as a part of them by the succeeding.

Suppose that a century ago, in the year 1802, certain French Academicians, believing like everyone else then in the 'laws of nature,' were invited, in the light of the best scientific knowledge of the day, to name the most grotesque and outrageous violation of them which the human mind could conceive. I may suppose them to reply, 'if a cartload of black stones were to tumble out of the blue sky above us, before our eyes, in this very France, we should call *that* a violation of the laws of nature, indeed!' Yet the next year, not one, but many, cartloads of black stones did tumble out of the blue sky, not in some far off land, but in France itself.

It is of interest to ask what became of the 'laws of nature' after such a terrible blow. The 'laws of nature' were adjusted, and after being enlarged by a little patching, so as to take in the new fact, were found to be just as good as ever! So it is always; when the miracle *has* happened, then and only then it becomes most clear that it was no miracle at all, and that no 'law of nature' has been broken.

Applying the parable to ourselves then, how shall we deal with new 'facts' which are on trial, things perhaps not wholly demonstrated, yet partly plausible? During the very last generation hypnotism was such a violation of natural law. Now it is a part of it. What shall we say, again, about telepathy, which seemed so absurd to most of us a dozen years ago? I do not say there is such a thing now, but I would like to take the occasion to express my feeling that Sir William Crookes, as president of the British Association, took the right, as he took the courageous course, in speaking of it in the terms he did. I might cite other things, the objects of ridicule only a few years ago, of debate now, but which have not all found supporters who possess the courage of their convictions.

The lesson for us in dealing with them is not that we should refuse to believe on the one hand, and sneer at everything that is on trial; for this, though a very general and safe procedure, is not one to be recommended to those of us who have some higher ideal than acquiescence with the current belief.

The lesson for us is that we must not consider that anything is absolutely settled or true.

This is not to say that we are to be blown about by every wind of scientific doctrine. It is to be understood as a practical rule of life that we must act with the majority where our faith does not compel us to do otherwise; but it seems to me that we must always keep ready for use somewhere; in the background of our mind, possibly, but somewhere; the perhaps trite notion that we know nothing absolutely or in its essence; and remember that though trite it is always true, and to be kept as a guide at every turning of the scientific road, when we can not tell what is coming next.

How many doctrines of our own day will stand the light of the next century? What will they be saying of our doctrine of evolution *then?* I do not know; but let me repeat what I have said elsewhere, that the truths of the scientific church are not dogmas, but something put forward as provisional only, and which her most faithful children are welcome to disprove if they can. I believe that science as a whole is advancing with hitherto unknown rapidity, but that the evidence of this advance is not in reasoning, but in the observation that our doctrine is proving itself, by the fact that through its aid Nature obeys us more and more, as I certainly believe it does.

Never let us forget, however, that man, being the servant and interpreter of nature, as Bacon says, can do and understand so much, and so much only, as he has *observed* of the course of nature, and that beyond this he neither knows anything nor can do anything. No walk along 'the high priori road' will take him where he wants to go, and no 'law of nature' will certainly help him.

But these 'laws', having authority only as far as they are settled by evidence and by observation alone, it may be a just inquiry as to what constitutes observation and, above all, who judges the evidence.

If the kinetic theory of gases, for instance, is a matter of inference rather than of observation, are we sure that we have a better guarantee for it than a previous century had for phlogiston? Our good opinion of ourselves, as compared with our scientific fathers, makes us think we have. Certainly I think myself that we have; and yet, remember, it is the same human nature which judged that evidence then, that judges this evidence now, and remember that however rapidly science changes, human nature remains very much the same, and always has a good conceit of itself.

While we are venturing to utter truisms, I repeat, let us take once more this one, home to ourselves, that there is a great deal of this 'human nature' even in the best type of the scientific man, and that we of this twentieth century share it, with our predecessors, on whom we look pityingly, as our successors will look on us.

Let us repeat, and repeat once more, that though nature be external to ourselves, the so-called 'laws of nature' are from within—laws of our own minds—and a simple product of our human nature. Let us agree that the scientific imagination can suggest questions to put to nature, but not her answers. Let us read Bacon again, and agree with him that we understand only what we have observed. Finally let us add that we never understand even that, in the fullness of its meaning, for remember that of all the so-called laws of nature, the most constantly observed and the most intimately and personally known to us are those of life and death—and how much do we know about the meaning of *them?*

— 1348 —

THE FIRE WALK CEREMONY IN TAHITI.

BY

S. P. LANGLEY.

FROM THE SMITHSONIAN REPORT FOR 1901, PAGES 539-544
(WITH PLATES I-III).

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1902.

THE FIRE WALK CEREMONY IN TAHITI.[a]

By S. P. Langley.

The very remarkable description of the fire walk collected by Mr. Andrew Lang and others had aroused a curiosity in me to witness the original ceremony, which I have lately been able to gratify in a visit to Tahiti.

Among these notable accounts is one by Colonel Gudgeon, British resident at Raratonga, describing the experiment by a man from Raiatea, and also a like account of the Fiji fire ceremony from Dr. T. M. Hocken, whose article is also quoted in Mr. Lang's paper on the "Fire Walk," in the Proceedings of the Society for Psychical Research, February, 1900. This extraordinary rite is also described by Mr. Fraser in the Golden Bough, and by others.

I had heard that it was performed in Tahiti in 1897, and several persons there assured me of their having seen it, and one of them of his having walked through the fire himself under the guidance of the priest, Papa-Ita, who is said to be one of the last remnants of a certain order of the priesthood of Raiatea, and who had also performed the rite at the island of Hawaii some time in the present year, of which circumstantial newspaper accounts were given, agreeing in all essential particulars with those in the accounts already cited. According to these, a pit was dug in which large stones were heated red-hot by a fire which had been burning many hours. The upper stones were pushed away just before the ceremony, so as to leave the lower stones to tread upon, and over these, "glowing red-hot" (according to the newspaper accounts), Papa-Ita had walked with naked feet, exciting such enthusiasm that he was treated with great consideration by the whites, and by the natives as a god. I found it commonly believed in Tahiti that anyone who chose to walk after him, European or native, could do so in safety, secure in the magic which he exercises, if his instructions were exactly followed. Here in Tahiti, where he had "walked" four years before, it was generally believed among the natives, and even among the Europeans present who had seen the ceremony, that if anyone turned around to look back he immediately was burned, and I was told that all those who followed him through the fire were expected not to turn until they had reached the other side in safety, when he again entered the fire and led them back by the path by which he had come. I was further told by several who had tried it that the heat was not felt upon the feet, and that when shoes were worn the soles were not burned (for those who followed the priest's directions), but it was added by all that much heat was felt about the head.

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[a] Reprinted from Nature, London, August 22, 1901.

Such absolutely extraordinary accounts of the performance had been given to me by respectable eyewitnesses and sharers in the trial, confirming those given in Hawaii, and, in the main, the cases cited by Mr. Lang, that I could not doubt that if all these were verified by my own observation, it would mean nothing less to me than a departure from the customary order of nature and something very well worth seeing indeed.

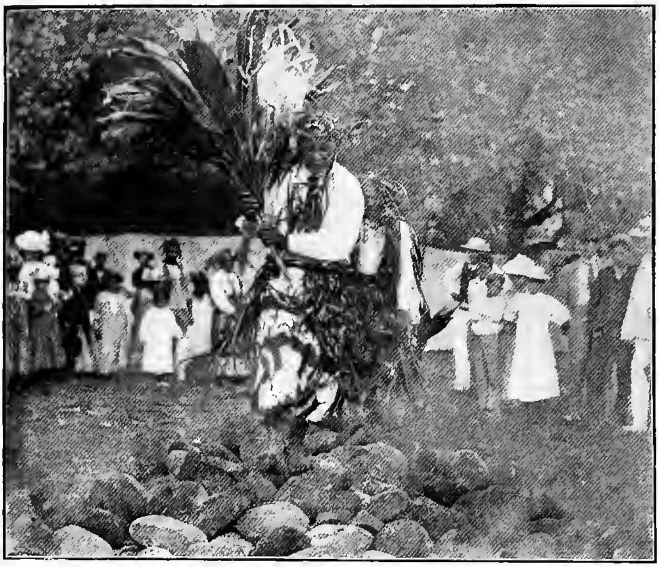
I was glad, therefore, to meet personally the priest, Papa-Ita. He is the finest looking native that I have seen; tall, dignified in bearing, with unusually intelligent features. I learned from him that he would perform the ceremony on Wednesday, July 17, the day before the sailing of our ship. I was ready to provide the cost of the fire, if he could not obtain it otherwise, but this proved to be unnecessary.

Papa-Ita himself spoke no English, and I conversed with him briefly through an interpreter. He said that he walked over the hot stones without danger by virtue of spells which he was able to utter and by the aid of a goddess (or devil as my interpreter had it), who was formerly a native of the islands. The spells, he said, were something which he could teach another. I was told by others that there was a still older priest in the island of Raiatea, whose disciple he was, although he had pupils of his own, and that he could "send his spirit" to Raiatea to secure the permission of his senior priest if necessary.

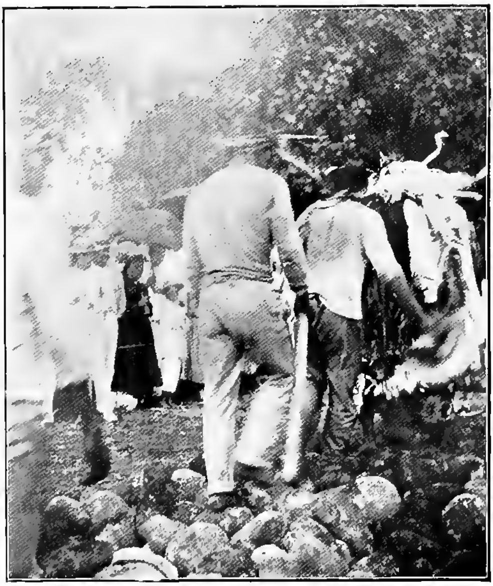
In answer to my inquiry as to what preparations he was going to make for the rite in the two or three days before it, he said he was going to pass them in prayer.

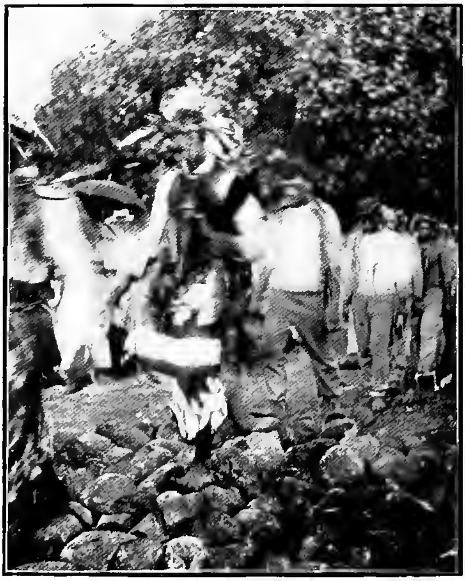
The place selected for the ceremony fortunately was not far from the ship. I went there at noon and found that a large shallow pit or trench had been dug, about 9 by 21 feet and about 2 feet deep. Lying nearby was a pile containing some cords of rough wood and a pile of rounded water-worn stones, weighing, I should think, from 40 to 80 pounds apiece. They were, perhaps, 200 in number, and all of porous basalt, a feature the importance of which will be seen later. The wood was placed in the trench, the fire was lighted and the stones heaped on it, as I was told, directly after I left, or at about 12 o'clock.

At 4 p. m. I went over again and found the preparations very nearly complete. The fire had been burning for nearly four hours. The outer stones touched the ground only at the edges of the pile, where



FIRE WALK CEREMONY IN TAHITI.





FIRE WALK CEREMONY IN TAHITI.

they did not burn my hand, but as they approached the center the stones were heaped up into a mound three or four layers deep, at which point the lowest layers seen between the upper ones were visibly red-hot. That these latter were, nevertheless, sending out considerable heat there could be no question, though the topmost stones were certainly not red-hot, while those at the bottom were visibly so and were occasionally splitting with loud reports, while the flames from the burned wood near the center of the pile passed up in visible lambent tongues, both circumstances contributing to the effect upon the excited bystanders.

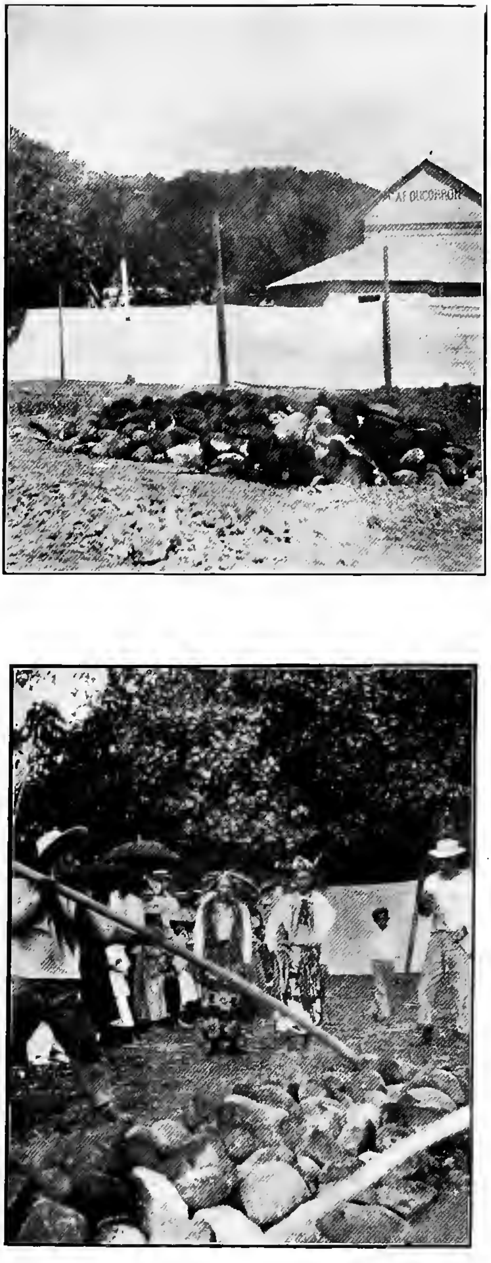
The upper stones, I repeat, even where the topmost were presently removed, did not show any glow to the eye, but were unquestionably very hot and certainly looked unsafe for naked feet. Native feet, however, are not like European ones, and Mr. Richardson, the chief engineer of the ship, mentioned that he had himself seen elsewhere natives standing unconcerned with naked feet on the cover of pipes conveying steam at about 300° F., where no European foot could even lightly rest for a minute. The stones then were hot. The crucial question was, How hot was the upper part of this upper layer on which the feet were to rest an instant in passing? I could think of no ready thermometric method that could give an absolutely trustworthy answer, but I could possibly determine on the spot the thermal equivalent of one of the hottest stones trodden on. (It was subsequently shown that the stone might be much cooler at one part than another.) Most obviously, even this was not an easy thing to do in the circumstances, but I decided to try to get at least a trustworthy approximation. By the aid of Chief Engineer Richardson, who attended with a stoker and one of the quartermasters, kindly detailed at my request by the ship's master, Captain Lawless, I prepared for the rough but conclusive experiment presently described.

It was now nearly forty minutes after 4, when six acolytes (natives), wearing crowns of flowers, wreathed with garlands and bearing poles nearly 15 feet long, ostensibly to be used as levers in toppling over the upper stones, appeared. They were supposed to need such long poles because of the distance at which they must stand on account of the heat radiated from the pile, but I had walked close beside it a moment before and satisfied myself that I could have manipulated the stones with a lever of one-third the length, with some discomfort, but with entire safety. Some of the uppermost stones only were turned over, leaving a superior layer, the long poles being needlessly thrust down between the stones to the bottom, where two of them caught fire at their extremities, adding very much to the impression that the exposed layer of stones was red hot, when in fact they were not, at least to the eye. These long poles and the way they were handled were, then, a part of the ingenious "staging" of the whole spectacle.

Now the most impressive part of the ceremony began. Papa-Ita, tall, dignified, flower-crowned, and dressed with garlands of flowers, appeared with naked feet and with a large bush of "ti" leaves in his hands, and after going partly around the fire each way, uttering what seemed to be commands to it, went back, and, beating the stones nearest him three times with the ti leaves, advanced steadily, but with obviously hurried step, directly over the central ridge of the pile. Two disciples, similarly dressed, followed him, but they had not the courage to do so directly along the heated center. They followed about halfway between the center and the edge, where the stones were manifestly cooler, since I had satisfied myself that they could be touched lightly with the hand. Papa-Ita then turned and led the way back, this time with deliberate confidence, followed on his return by several new disciples, most of them not keeping exactly in the steps of the leader, but obviously seeking cooler places. A third and fourth time Papa-Ita crossed with a larger following, after which many Europeans present walked over the stones without reference to the priest's instructions. The natives were mostly in their bare feet. One wore stockings. No European attempted to walk in bare feet, except in one case—that of a boy, who, I was told, found the stones too hot and immediately stepped back.

The mise en scène was certainly noteworthy. The site near the great ocean breaking on the barrier reefs, the excited crowd talking about the "red-hot" stones, the actual sight of the hierophant and his acolytes making the passage along the ridge where the occasional tongues of flame were seen at the center, with all the attendant circumstances, made up a scene in no way lacking in interest. Still, the essential question as to the actual heat of these stones had not yet been answered, and after the fourth passage I secured Papa-Ita's permission to remove from the middle of the pile one stone, which, from its size and position, every foot had rested upon in crossing and which was undoubtedly at least as hot as any one of those trodden on. It was pulled out by my assistants with difficulty, as it proved to be larger than I had expected, it being of ovoid shape, with the lower end in the hottest part of the fire. I had brought over the largest wooden bucket which the ship had and which was half filled with water, expecting that this would cover the stone, but it proved to be hardly enough. The stone caused the water to rise nearly to the top of the bucket, and it was thrown into such violent ebullition that a great deal of it boiled over and escaped weighing. The stone was an exceedingly bad conductor of heat, for it continued to boil the water for about twelve minutes, when, the ebullition being nearly over, it was removed to the ship and the amount of evaporated water measured.

Meanwhile others, as I have said, began to walk over the stones without any reference to the ceremony prescribed by Papa-Ita, and



FIRE WALK CEREMONY IN TAHITI.

three or four persons, whom I personally knew on board the ship, did so in shoes, the soles of which were not burned at all. One of the gentlemen, however, who crossed over with unburned shoes, showed me that the ends of his trousers had been burnt by the flames which leaped up between the stones, and which at all times added so much to the impressiveness of the spectacle; and there was no doubt that anyone who stumbled or got a foot caught between the hot stones might have been badly burned. United States Deputy Consul Ducorran, who was present, remarked to me that he knew that Papa-Ita had failed on a neighboring island, with stones of a marble-like quality, and he offered to test the heat of these basaltic ones by seeing how long he could remain on the hottest part of the pile, and he stood there, in my sight, from eight to ten seconds before he felt the heat through the thin soles of his shoes beginning to be unpleasantly warm.

A gentleman present asked Papa-Ita why he did not give an exhibit that would be convincing by placing his foot, even for a few seconds, between two of the red-hot stones which could be seen glowing at the bottom of the pile, to which Papa-Ita replied with dignity, "My fathers did not tell me to do it that way." I asked him if he would hold one of the smaller, upper hot stones in his hand. He promised to do so, but he did not do it.

The outer barriers were now removed and a crowd of natives pressed in. I, who was taking these notes on the spot, left, after assuring myself that the stones around the edge of the pit were comparatively cold, although the center was no doubt very hot, and those below red-hot. The real question is, I repeat, How hot were those trodden on? and the answer to this I was to try to obtain after measuring the amount of water boiled away.

On returning to the ship this was estimated from the water which was left in the bucket (after allowing for that spilled over) at about 10 pounds. The stone, which it will be remembered was one of the hottest, if not the hottest, in the pile, was found to weigh 65 pounds, and to have evaporated this quantity of water. It was, as I have said, a volcanic stone, and on minuter examination proved to be a vesicular basalt, the most distinctive feature of which was its porosity and non-conductibility, for it was subsequently found that it could have been heated red-hot at one end, while remaining comparatively cool at the top. I brought a piece of it to Washington with me and there determined its specific gravity to be 2.39, its specific heat 0.19, and its conductivity to be so extremely small that one end of a small fragment could be held in the hand while the other was heated indefinitely in the flame of a blowpipe, almost like a stick of sealing-wax. This partly defeated the aim of the experiment (to find the temperature of the upper part of the stone), since only the mean temperature was found. This mean temperature of the hottest stone of the upper layer, as deduced from the above data, was about 1,200° F., but the temperature of the surface must have been indefinitely lower. The temperature at which such a stone begins to show a dull red in daylight is, so far as I am aware, not exactly determined, but is approximately 1,300 to 1,400° F.

To conclude, I could entertain no doubt that I had witnessed substantially the scenes described by the gentlemen cited, and I have reason to believe that I saw a very favorable specimen of a fire walk.

It was a sight well worth seeing. It was a most clever and interesting piece of savage magic, but from the evidence I have just given I am obliged to say (almost regretfully) that it was not a miracle.

— 1385 —

"GOOD SEEING."

BY

S. P. LANGLEY.

FROM THE SMITHSONIAN REPORT FOR 1902, PAGES 193–195
(WITH PLATE I).

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1903.

"GOOD SEEING."[a]

By S. P. LANGLEY.

Everyone who has used a telescope knows that our atmosphere is forever in pulsating motion, and troubling our vision of the heavenly bodies, during the most cloudless day or night, so that observatories are put even on high mountains, to get rid of the disturbances in this atmosphere, which tend to make the image of every object tremulous and indefinite, and to prevent what the astronomer terms "good seeing."

I desire to speak to the academy about a device which I have recently essayed for preventing this universally known and dreaded "boiling" of the telescopic image, a difficulty which has existed always and everywhere since telescopes have been in use, and which has seemed so insurmountable that I believe it has hardly ever been thought of as subject to correction.

Hitherto it has been the endeavor of astronomers, so far as I know, to secure a more tranquil image by keeping the air in the telescope tube, through which the rays pass, as quiet as possible, and for this purpose the walls of the tube have been made nonconducting, and extreme pains have been taken not to set up currents in the tube. With these precautions the "seeing" is perhaps a little better (but very little) than if none were used at all, the main difficulty having been always found insurmountable.

I have been led for some years to consider the conditions under which this "boiling" presents itself. It is not necessarily due to a high temperature of the external air, for the most perfect definition I have ever seen of any terrestrial object was obtained by me long since in the Harvard College Observatory at Cambridge, with its great equatorial telescope, when, on the hottest day that I ever knew in a New England summer, I directed it with a high power on the distant "south mark," which I expected to find almost indistinguishable from the "boiling." I remember my extreme surprise when, under a magnifying power of 300, I found the image as still as the lines of an engraving. This was an extraordinary exception to ordinary experience, and led me to take an interest in the subject. I have since pursued an inquiry to which this circumstance first directed my attention, and I have done so at all altitudes, at one time residing on Ætna for this purpose, noting that even on high mountains telescopic vision was so far from being always clear that it was sometimes even much worse than at sea level.

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[a] A paper presented to the National Academy of Sciences, November 12, 1902.

I have since come to the important conclusion that while the ordinary "boiling" is due to all the air between us and the sun or star through which the rays pass, the greater portion of it is due to the air immediately near us, probably within a few hundred yards or even feet from the telescope, and this has led me to ask whether it was not possible that some way to act upon this air could be found. Its nonuniformity leads to deformations of the image too complex to analyze here, which are caused not only by lateral vibrations of the cone of rays but by its elongation and contraction.

For this purpose I have, within the last few months, been making experiments at the Smithsonian Astrophysical Observatory; first with a horizontal tube having three successive walls with air spaces between, which was intended to give the maximum security which freedom from changes of temperature could afford. This observatory, being principally concerned with rays best studied in an image formed by reflection, has no large dioptric telescope, on which account these experiments have been made with a reflector. I have no reason to suppose, however, that they will not be equally successful with a dioptric telescope.

A large part of the "boiling" of the image is due to air without the tube, but a not unimportant part to the air within it; and in the preliminary experiments the air, kept still in the tube by treating it with the ordinary precautions, was found to have little effect on the ordinary "boiling" of the image, which so seriously prejudices the definition. An image-forming mirror, fed by a coelostat, was placed at the end of this triple-walled tube, which was itself sheltered by a canvas tent and contained the stillest air of the most uniform temperature which could be obtained. The "boiling" was but little diminished merely by inclosing the beam by this tube, which was only what had been anticipated from the ordinary experience of all astronomers.

The device which I had determined to try was one of a paradoxical character, for it proposed to substitute for this still air, which gave the usual troubled image, agitated air which it was hoped would give a still image.[a] For the purpose of this new experiment, the horizontal telescope using a reflector of 40 feet focus, fed by a coelostat through the above tube, was connected with a fan run by an electric motor, which was arranged to draw out the air from the inner tube at the same time that it forced air in at different points in its length, so as to

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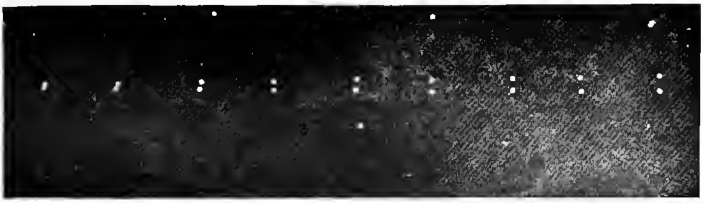
[a] I may mention here that my lamented friend, Henry C. Draper, once showed me that agitating the contents of a bisulphide of carbon prism improved its definition.

1.



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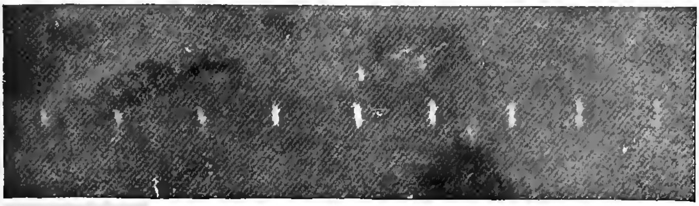

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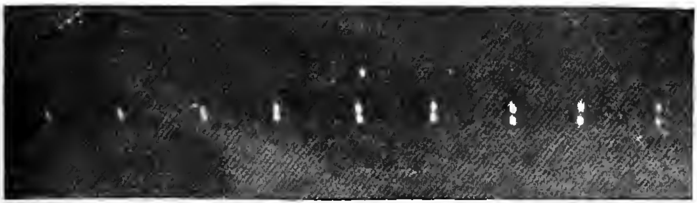

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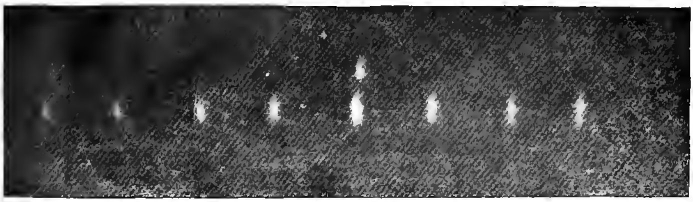

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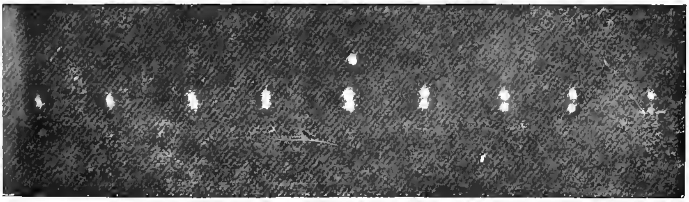


EXPERIMENTS ON ARTIFICIAL GOOD SEEING, SMITHSONIAN ASTROPHYSICAL OBSERVATORY.

No. 1. The object, a series of artificial double stars.
2. Image of No. 1, without stirring. } Exposure, three minutes.
3. Image of No. 1, with stirring. }
4. Image of No. 1, without stirring. } Exposure, four minutes.
5. Image of No. 1, with stirring. }

thus violently disturb and churn the air along all the path of the beam from the coelostat to the solar image.

This first experiment gratifyingly reduced the "boiling" and produced an incontestibly stiller image than when still air was used. As a further test, a series of artificial double stars was now provided, and the concave mirror, acting both as collimator and objective, brought the images to focus, where they were examined with an eyepiece. With the stillest air obtainable the images were not sharp, and only the coarsest doubles were resolvable. Then the blower was started and the definition immediately became sharp. Violently stirring the air in the tube, then, eliminates all or nearly all the "boiling" of the stellar image which arises within the tube itself when using ordinary still air. This experiment concerned the air within the horizontal tube only.

I have next taken up the solar image formed by the mirror in the above tube. This is clearly improved by the stirring. I have also wished to try a tube something like a prolonged dew cap, but which is arranged to be inclined toward the sun. The air in both can be stirred together, but experiment has not yet gone far enough to demonstrate whether it has, as is hoped, any superiority commensurate with the special mechanical difficulties involved.

I am not prepared to give quantitative estimates, which I hope to furnish later, but all observers to whom I have shown these early results on the sun have agreed, that if the "boiling" was not wholly cured, what remained was but a small fraction of that obtained with still air. I have not completed these experiments, which I am still pursuing at the observatory, but they seem to me to give promise of an improvement of universal interest to observers, which justified the making of this early announcement. I had hoped to have shown the Academy some photographs of the sun taken, first, in the ordinary way, and again, with the churned air, but the condition both of the sun and of the sky of late has prevented my obtaining them. I can, to my regret, only give here a photograph of the images of the artificial double stars as seen through ordinary conditions, as distinguished from those here mentioned, of artificial "good seeing."

EXPERIMENTS WITH THE LANGLEY AERODROME.

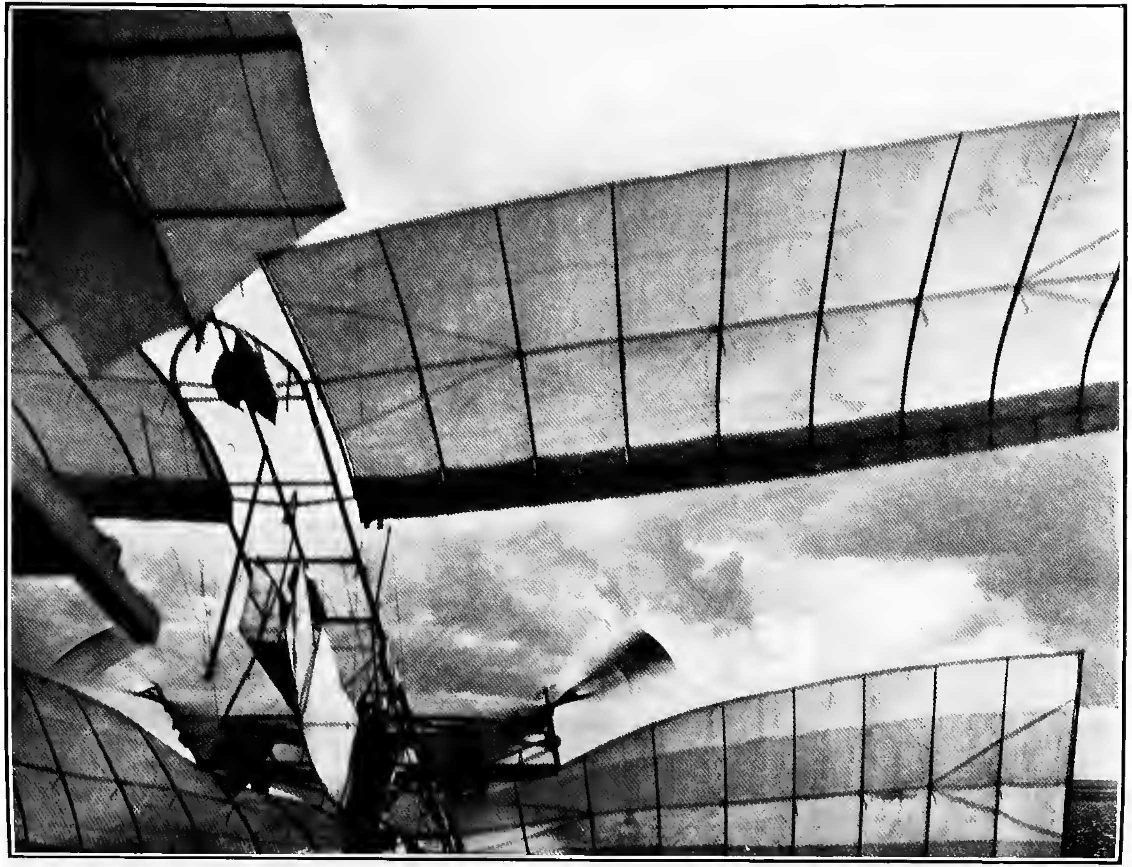
BY

S. P. LANGLEY.

FROM THE SMITHSONIAN REPORT FOR 1904, PAGES 113–125
(WITH PLATE I).

(No. 1597.)

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1905.



INSTANTANEOUS PHOTOGRAPH OF THE LAUNCH OF OCTOBER 7, 1903.

EXPERIMENTS WITH THE LANGLEY AERODROME.

By S. P. Langley.

The experiments undertaken by the Smithsonian Institution upon an aerodrome, or flying machine, capable of carrying a man have been suspended from lack of funds to repair defects in the launching apparatus without the machine ever having been in the air at all. As these experiments have been popularly, and of late repeatedly, represented as having failed on the contrary, because the aerodrome could not sustain itself in the air I have decided to give this brief though late account, which may be accepted as the first authoritative statement of them.

It will be remembered that in 1896 wholly successful flights of between one-half and one mile by large steam-driven models, unsupported except by the mechanical effects of steam engines, had been made by me. In all these the machine was first launched into the air from "ways," somewhat as a ship is launched into the water, the machine resting on a car that ran forward on these ways, which fell down at the extremity of the car's motion, releasing the aerodrome for its free flight. I mention these details because they are essential to an understanding of what follows, and partly because their success led me to undertake the experiments on a much larger scale I now describe.

In the early part of 1898 a board, composed of officers of the Army and Navy, was appointed to investigate these past experiments with a view to determining just what had been accomplished and what the possibilities were of developing a large-size man-carrying machine for war purposes. The report of this board being favorable, the Board of Ordnance and Fortification of the War Department decided to take up the matter, and I having agreed to give without compensation what time I could spare from official duties, the Board allotted $50,000 for the development, construction, and test of a large aerodrome, half of which sum was to be available immediately and the remainder when required. The whole matter had previously been laid before the Board of Regents of the Smithsonian Institution, who had authorized me to take up the work and to use in connection with it such facilities of the Institution as were available.

Before consenting to undertake the construction of this large machine, I had fully appreciated that owing to theoretical considerations, into which I do not enter, it would need to be relatively lighter than the smaller one; and later it was so constructed, each foot of sustaining surface in the large machine carrying nearly the same weight as each foot in the model. The difficulties subsequently experienced with the larger machine were, then, due not to this cause, but to practical obstacles connected with the launching, and the like.

I had also fully appreciated the fact that one of the chief difficulties in its construction would lie in the procuring of a suitable engine of sufficient power and, at the same time, one which was light enough. (The models had been driven by steam engines whose water supply weighed too much for very long flights.) The construction of the steam engine is well understood, but now it would become necessary to replace this by gas engines, which for this purpose involve novel difficulties. I resolved not to attempt the task of constructing the engine myself, and had accordingly entered into negotiations with the best engine builders in this country, and after long delay had finally secured a contract with a builder who, of all persons engaged in such work, seemed most likely to achieve success. It was only after this contract for the engine had been signed that I felt willing to formally undertake the work of building the aerodrome.

The contract with the engine builder called for an engine developing 12 brake horsepower, and weighing not more than 100 pounds, including cooling water and all other accessories, and with the proviso that a second engine, exactly like this first one, would be furnished on the same terms. The first engine was to be delivered before the close of February, 1899, and the frame of the aerodrome with sustaining surfaces, propellers, shafting, rudders, etc., was immediately planned, and now that the engine was believed to be secured, their actual construction was pushed with the utmost speed. The previous experiments with steam-driven models, which had been so successful, had been conducted over the water, using a small house-boat having a cabin for storing the machine, appliances and tools, on top of which was mounted a track and car for use in launching. As full success in launching these working models had been achieved after several years spent in devising, testing and improving this plan, I decided to follow the same method with the large machine, and accordingly designed and had built a house-boat, in which the machine could not only be stored, but which would also furnish space for workshops, and on the top of which was mounted a turntable and track for use in launching from whatever direction the wind might come.

Everything connected with the work was expedited as much as possible with the expectation of being able to have the first trial flight before the close of 1899, and time and money had been spent on the aerodrome, which was ready, except for its engine, when the time for the delivery of this arrived. But now the builder proved unable to complete his contract, and, after months of delay, it was necessary to decrease the force at work on the machine proper and its launching appliances until some assurance could be had of the final success of the engine. During the spring and summer of 1899, while these delays were being experienced in procuring suitable engines, former experiments on superposed wing surfaces were continued, time was found for overhauling the two steam-driven models which had been used in 1896, and the small house boat was rebuilt so that further tests of these small machines might be made in order to study the effect of various changes in the balancing and the steering, equilibrium preserving and sustaining appliances, and the months of June, July, and a portion of August were spent in actual tests of these machines in free flight.

A new launching apparatus following the general plan of the former overhead one, but with the track underneath it, was built for the models, and it was used most successfully in these experiments, more than a dozen flights in succession being made with it, while in every case it worked without delay or accident. As soon as these tests with the models on this underneath launching apparatus were completed, that for the large machine was built as an exact duplicate, except for the enlargement, and with some natural confidence that what had worked so perfectly on a small scale would work fairly on a large one.

It was recognized from the very beginning that it would be desirable in a large machine to use "superposed" sustaining surfaces (that is, with one wing above another) on account of their superiority so far as the relation of strength to weight is concerned, and from their independence of guy wiring; and two sets of superposed sustaining surfaces of different patterns were built and experimented with in the early tests. These surfaces proved, on the whole, inferior in lifting power, though among compensating advantages are the strength of a "bridge" construction which dispenses with guy wires coming up from below, which, in fact, later were the cause of disaster in the launching.

It was finally decided to follow what experiment had shown to be successful, and to construct the sustaining surfaces for the large machine after the "single-tier" plan. This proved to be no easy task, since in the construction of the surfaces for the small machines the main and cross ribs of the framework had been made solid, and, after steaming, bent and dried to the proper curvature, while it was obvious that this plan could not be followed in the large surfaces on account of the necessity, already alluded to, of making them relatively lighter than the small ones, which were already very light. After the most painstaking construction, and tests of various sizes and thicknesses of hollow square, hollow round, I-beam, channel, and many other types of ribs, I finally devised a type which consisted of a hollow box form, having its sides of tapering thickness, with the thickest part at the point midway between contiguous sides and with small partitions placed inside every few inches in somewhat the same way that nature places them in the bamboo. These various parts of the rib (corresponding to the quill in a wing) were then glued and clamped together, and after drying were reduced to the proper dimensions and the ribs covered with several coats of a special marine varnish, which it had been found protected the glued joints from softening, even when they were immersed in water for twenty-four hours.

Comparative measurements were made between these large cross ribs, 11 feet long, and a large quill from the wing of a harpy eagle, which is probably one of the greatest wonders that nature has produced in the way of strength for weight. These measurements showed that the large, 11-foot ribs ("quills") for the sustaining surfaces of the large machine were equally as strong, weight for weight, as the quill of the eagle; but much time was consumed in various constructions and tests before such a result was finally obtained.

During this time a model of the large machine, one-fourth of its linear dimensions, was constructed, and a second contract was made for an engine for it. The delay with the large engine was repeated with the small one, and in the spring of 1900 it was found that both contract engines were failures for the purpose for which they were intended, as neither one developed half of the power required for the allotted weight.

I accordingly again searched all over this country, and, finally, accompanied by an engineer (Mr. Manly), whose services I had engaged, went to Europe, and there personally visited large builders of engines for automobiles, and attempted to get them to undertake the construction of such an engine as was required. This search, however, was fruitless, as all of the foreign builders, as well as those of this country, believed it impossible to construct an engine of the necessary power and as light as I required (less than 10 pounds to the horsepower without fuel or water). I was therefore forced to return to this country and to consent most reluctantly, even at this late date, to have the work of constructing suitable engines undertaken in the shops of the Smithsonian Institution, since, as I have explained, the aerodrome frame and wings were already constructed. This work upon the engines began here in August, 1900, in the immediate care of Mr. Manly. These engines were to be of nearly double the power first estimated and of little more weight, but this increased power and the strain caused by it demanded a renewal of the frame as first built, in a stronger and consequently in a heavier form, and the following sixteen months were spent in such a reconstruction simultaneously with the work on the engines.

The flying weight of the machine complete, with that of the aeronaut, was 830 pounds; its sustaining surface, 1,040 square feet. It therefore was provided with slightly greater sustaining surface and materially greater relative horsepower than the model subsequently described which flew successfully. The brake horsepower of the engine was 52; the engine itself, without cooling water, or fuel, weighed approximately 1 kilogram to the horsepower. The entire power plant, including cooling water, carburetor, battery, etc., weighed materially less than 5 pounds to the horsepower. Engines for both the large machine and the quarter-size model were completed before the close of 1901, and they were immediately put in their respective frames and tests of them and their power-transmission appliances were begun.

It is well here to call attention to the fact that although an engine may develop sufficient power for the allotted weight, yet it is not at all certain that it will be suitable for use on a machine which is necessarily as light as one for traversing the air, for it would be impossible to use, for instance, a single cylinder gasoline engine in a flying machine unless it had connected to it prohibitively heavy flywheels. These facts being recognized, the engines built in the Smithsonian shops were provided with five cylinders, and it was found upon test that the turning effect received from them was most uniform, and that, by suitable balancing of rotating and reciprocating parts, they could be made to work so that there was practically no vibration, even when used in the very light frames of the aerodromes.

The engine is not all the apparatus connected with the development and delivery of power, for obviously there must be shafts, bearings, and in the present case there were also gears; and all of these parts must necessarily be phenomenally light, while all of the materials must be capable of withstanding repeated and constant strains far beyond their elastic limit. It is also evident to anyone having familiarity with such constructions that it is most difficult to keep the various bearings, shafts, gears, etc., in proper alignment without adding excessive weight, and also that when these various parts once get out of alignment when subject to strain, the disasters which are caused render them unfit for further use.

The engines themselves were successfully completed before the close of 1901, and were of much more power than those originally designed; but nearly a year and a half had been spent not only in their completion, but in properly coordinating the various parts of the frame carrying them, repairing the various breakages, assembling, dismounting, and reassembling the various parts of the appliances, and in general rebuilding the frame and appurtenances to correspond in strength to the new engines.

There are innumerable other details, for the whole question is one of details. I may, however, particularly mention the carburetors, which form an essential part of every gas engine, and such giving fair satisfaction for use in automobiles were on the market at the time, yet all of them failed to properly generate gas when used in the tests of the engine working in the aerodrome frame, chiefly because of the fact that the movement of the engine in this light frame must be constant and regular or the transmission appliances are certain of distortion. It was, therefore, necessary to devise carburetors for the aerodrome engines which would meet the required conditions, and more than half a dozen were constructed which were in advance of anything then on the market, and yet were not good enough to use in the aerodrome, before a satisfactory one was made. These experiments were made in the shop, but with an imitation of all the disturbing influences which would be met with in the actual use of the machine in the air, so as to make certain, as far as possible, that the first test of the machine in free flight would not be marred by mishaps or unseen contingencies in connection with the generation and use of power.

It is impossible for anyone who has not had experience with such matters to appreciate the great amount of delay which experience has shown is to be expected in such experiments. Only in the spring of 1903, and after two unforeseen years of assiduous labor, were these new engines and their appurtenances, weighing altogether less than 5 pounds to the horsepower and far lighter than any known to be then existing, so coordinated and adjusted that successive shop tests could be made without causing injury to the frame, its bearings, shafts, or propellers.

And now everything seemed to be as nearly ready for an experiment as could be, until the aerodrome was at the location at which the experiments were to take place. The large machine and its quarter-size counterpart were accordingly placed on board the large house boat, which had been completed some time before and had been kept in Washington as an auxiliary shop for use in the construction work, and the whole outfit was towed to a point in the Potomac River, here 3 miles wide, directly opposite Widewater, Va., and about 40 miles below Washington and midway between the Maryland and Virginia shores, where the boat was made fast to moorings which had previously been placed in readiness for it.

Although extreme delays had already occurred, yet they were not so trying as the ones which began immediately after the work was thus transferred to the lower Potomac.

The object in constructing the quarter-size counterpart of the large machine was to duplicate in it the balancing and relative proportions of power, surface, etc., that had been arranged in the large one, so that a test of it might be made which would determine whether the large machine should be tried as arranged or the balancing and other arrangements modified. The launching apparatus, which had proved so eminently successful with the original steam-driven models in 1896, was considered a thing so well tested that it had, as I have stated, been duplicated on a suitable scale for use with the large aerodrome, and it was felt that if this apparatus were exactly similar to the smaller one it would be the one appliance least likely to mar the experiments.

In order to test the quarter-size model it was necessary to remove its launching track from the top of the small house boat and place it upon the deck of the large boat, in order to have all the work go on at one place, as it was impossible, on account of its unseaworthiness, to moor the small house boat in the middle of the river.

While this transfer of the launching apparatus from the small boat to the large one was being made, the changed atmospheric conditions incident to a large body of water over which thick fogs hung a great portion of the time, from those of a well-protected shop on the land, began to manifest themselves in such ways as the rusting of the metal parts and fittings, and the consequent disarrangement of the adjustment of the necessarily very accurate pieces of apparatus connected with the ignition system of the engine. These difficulties might have partly been anticipated, but there were others concerning which the cause of the deterioration and disarrangement of certain parts and adjustments was not immediately detected, and consequently when short preliminary shop tests of the small machine were attempted just prior to launching it, it was found that the apparatus did not work properly, necessitating repairs and new constructions and consequent delay. Although the large house boat with the entire outfit had been moved down the river on July 14, 1903, it was not until the 8th of August that the test of the quarter-size model was made, and all of this delay was directly due to changed atmospheric conditions incident to the change in locality. This test of the model in actual flight was made on the 8th of August, 1903, when it worked most satisfactorily, the launching apparatus, as always heretofore, performing perfectly, while the model, being launched directly into the face of the wind, flew directly ahead on an even keel. The balancing proved to be perfect, and the power, supporting surface, guiding, and equilibrium-preserving effects of the rudder also. The weight of the model was 58 pounds, its sustaining surface 66 square feet, and the horsepower from 2½ to 3.

This was the first time in history, so far as I know, that a successful flight of a mechanically sustained flying machine was made in public.

The flight was not as long as had been expected, as it was found afterwards that one of the workmen, in his zeal to insure an especially good one, had overfilled the gasoline tank, which would otherwise have enabled a flight several times as long. However, as such a flight would have given absolutely no more data than the short one did, and as the delays in getting ready for testing the large machine had already far exceeded what was expected, it was thought best not to make any more tests with the small one, as all of the data which was desired had been procured, and it was accordingly stored away and every energy immediately concentrated in getting the large machine ready for its first test, which at that time seemed only a few days away.

During all these delays it may be remarked that we necessarily resided near the house boat, and therefore in a region of malaria, from whose attacks a portion of us suffered.

I have spoken of the serious delays in the test of the small machine caused by changed atmospheric conditions, but they proved to be almost negligible compared with what was later experienced with the large one. I have also alluded to the fact that the necessarily light ribs of the large sustaining wing surfaces were covered with several coats of a special marine varnish which many tests had shown enabled the glue to withstand submersion in water for more than twenty-four hours without being affected. This water test was made with a view to guarding against the joints of the ribs being softened when the machine came down into the water, as it was planned for it to do at the close of its flight, and these submersions had apparently shown that no trouble need be anticipated from the effects of the sustaining surfaces getting wet. It is an instance of the unpredictable delays which present themselves, that when preparations had been begun for the immediate trial of the large machine, already down the river, it was found that every one of the cross ribs had been rendered almost useless by the damp, though under shelter. As it would take months to build new ones, a temporary means of repairing them was used. There were other delays too numerous to mention, but chiefly incident to working over the water, some of the principal of which were due to storms dragging the house boat from its moorings and destroying auxiliary apparatus, such as launches, boats, rafts, etc., to say nothing of the time consumed in bringing workmen to and from the scene of the experiments. The propellers were even found to break under the strain of the actual engines in the open, though they had not done so in the shop, and this is mentioned as another instance of the numerous causes of trying delay which it was impossible to foresee.

Finally, however, on the 3d of September, everything seemed to be in readiness for the experiments, and the large aerodrome was accordingly placed in position and all orders given and arrangements made for a test that day. After stationing the various tugs, launches, etc., at their predetermined positions so that they might render any assistance necessary to the engineer or the aerodrome, in case it came down in the water at a point distant from the house boat, and after the photographers, with special telephoto cameras, had been stationed on the shore in order that photographs with their trigonometrical data might be obtained, from which speed, distance, etc., might be later determined, and when everyone was anxiously expecting the experiment, a delay occurred from one of the hardly predictable causes just mentioned in connection with the weather. An attempt was made to start the engine so that it might be running at its proper rate when the aerodrome was launched into free air after leaving the track, but the dry batteries used for sparking the engine, together with the entire lot of several dozen which were on hand as a reserve, had become useless from the dampness.

I have merely instanced some of these causes of failure when everything was apparently ready for the expected test, but only one who was on the spot and who had interest in the outcome could appreciate trials of this sort, and the delays of waiting for weather suitable for experiments.

It was found that every storm which came anywhere in the vicinity, immediately selected the river as its route of travel, and although a 10-mile wind on the land would not be an insurmountable obstacle during an experiment, yet the same wind on the river rendered it impossible to maintain the large house boat on an even keel and free from pitching and tossing long enough to make a test.

While speaking of the difficulties imposed by the weather, it should also be understood that to take the aerodrome in parts from under the shelter of the roof and assemble and mount it upon the upper works was a task requiring four or five hours, and that during this time a change in the weather was altogether likely to occur, and did repeatedly occur, sufficient to render the experiment impossible. Experience has shown, then, that the aerodrome should be sheltered by a building, in which it shall be at all times ready for immediate launching. During all the delay resulting from this and other causes—since it was never known on what day the experiment might take place—a great expense for tug boats waiting at a distance of 40 miles from the city, was incurred, and this was a part of the continuous drain on the pecuniary resources, which proved ultimately more fatal than any mishap to the apparatus itself.

Following the 3d of September, and after procuring new batteries, short preliminary tests inside the boat were made in order to make sure that there would be no difficulty in the running of the engine the next time a fair opportunity arrived for making a test of the machine in free flight. Something of the same troubles which had been met with in the disarrangement of the adjustments of the small engine was experienced in the large one, although they occurred in such a different way that they were not detected until they had caused damage in the tests, and these disarrangements were responsible for broken propellers, twisted shafts, crushed bearings, distorted framework, etc., which were not finally overcome until the 1st of October. After again getting everything in apparent readiness, there then ensued a period of waiting on the weather until the 7th of October (1903), when it became sufficiently quiet for a test, which I was now beginning to fear could not be made before the following season. In this, the first test, the engineer took his seat, the engine started with ease and was working without vibration at its full power of over 50 horse, and the word being given to launch the machine, the car was released and the aerodrome sped along the track. Just as the machine left the track, those who were watching it, among whom were two representatives of the Board of Ordnance,[a] noticed that the machine was jerked violently down at the front (being caught, as it subsequently appeared, by the falling ways),[b] and under the full power of its engine was pulled into the water, carrying with it its engineer. When the aerodrome rose to the surface it was found, that while the front sustaining surfaces had been broken by their impact with the water, yet the rear ones were comparatively uninjured. As soon as a full examination of the launching mechanism had been made, it was found that the front portion of the machine had caught on the launching car, and that the guy post, to which were fastened the guy wires which are the main strength of the front surfaces, had been bent to a fatal extent.

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[a] Major Macomb, of the Board of Ordnance, states in his report to the Board, that "the trial was unsuccessful because the front guy post caught in its support on the launching car and was not released in time to give free flight, as was intended, but, on the contrary, caused the front of the machine to be dragged downward, bending the guy post and making the machine plunge into the water about 50 yards in front of the house boat."

[b] This instantaneous photograph, taken from the boat itself and hitherto unpublished, shows the aerodrome in motion before it had actually cleared the house boat. On the left is seen a portion of a beam, being a part of the falling ways in which the front wing was caught, while the front wing itself is seen twisted, showing that the accident was in progress before the aerodrome was free to fly.

The machine, then, had never been free in the air, but had been pulled down as stated.

The disaster just briefly described had indefinitely postponed the test, but this was not all. As has been said before, the weather had become very cold and the so-called equinoctial storms being near it was decided to remove the house boat at the earliest time possible, but before it could be done, a storm came up and swept away all the launches, boats, rafts, etc., and in doing so completely demolished the greater part of them, so that when the house boat was finally removed to Washington, on the 15th of October, these appurtenances had to be replaced. It is necessary to remember that these long series of delays worked other than mere scientific difficulties, for a more important and more vital one was the exhaustion of the financial means for the work.

Immediately upon getting the boat to Washington the labor of constructing new sustaining surfaces was begun, and they were completed about the close of November. It was proposed to make a second attempt near the city, though in the meantime the ice had formed in the river. However, on the 8th of December, 1903, the atmosphere became very quiet shortly before noon and an immediate attempt was made at Arsenal Point, quite near Washington, though the site was unfavorable. Shortly after arriving at the selected point everything was in readiness for the test. In the meantime the wind had arisen and darkness was fast approaching, but as the funds for continuing the work were exhausted, rendering it impossible to wait until spring for more suitable weather for making a test, it was decided to go on with it if possible. This time there were on hand to witness the test the writer, members of the Board of Ordnance, and a few other guests, to say nothing of the hundreds of spectators who were waiting on the various wharves and shores. It was found impossible to moor the boat without a delay which would mean that no test could be made on account of darkness, so that it was held as well as possible by a tug, and kept with the aerodrome pointing directly into the wind, though the tide, which was running very strong, and the wind, which was blowing 10 miles an hour, were together causing much difficulty. The engine being started and working most satisfactorily, the order was given by the engineer to release the machine, but just as it was leaving the track another disaster, again due to the launching ways, occurred.[a]

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[a] Major Macomb again states in his official report to the Board: "The launching car was released at 4.45 p. m. * * * The car was set in motion and the propellers revolved rapidly, the engine working perfectly, but there was something wrong with the launching. The rear guy post seemed to drag, bringing the rudder down on the launching ways, and a crashing, rending sound, followed by the collapse of the rear wings, showed that the machine had been wrecked in the launching; just how it was impossible to see."

This time the rear of the machine, in some way still unexplained, was caught by a portion of the launching car, which caused the rear sustaining surfaces to break, leaving the rear entirely without support, and it came down almost vertically into the water. Darkness had come before the engineer, who had been in extreme danger, could aid in the recovery of the aerodrome, the boat and machine had drifted apart, and one of the tugs, in its zeal to render assistance, had fastened a rope to the frame of the machine in the reverse position from what it should have been attached and had broken the frame entirely in two. While the injury which had thus been caused seemed almost irreparable to one not acquainted with the work, yet it was found upon close examination that only a small amount of labor would be necessary in order to repair the frame, the engine itself being entirely uninjured. Had this accident occurred at an earlier period, when there were funds available for continuing the experiments, it would not have been so serious, for many accidents in shop tests had occurred which, while unknown to the general public, had yet caused greater damage and required more time for repair than in the present case. But the funds for continuing the work were exhausted, and it being found impossible to immediately secure others for continuing it, it was found necessary to discontinue the experiments for the present, though I decided to use, from a private fund, the small amount of money necessary to repair the frame so that it itself, together with its engine, which was entirely uninjured, might be available for further use if it should later prove possible, and that they themselves might be in proper condition to attest to what they really represent as an engineering achievement.

Entirely erroneous impressions have been given by the account of these experiments in the public press, from which they have been judged, even by experts; the impression being that the machine could not sustain itself in flight. It seems proper, then, to emphasize and to reiterate, with a view to what has just been said, that the machine has never had a chance to fly at all, but that the failure occurred on its launching ways; and the question of its ability to fly is consequently, as yet, an untried one.

There have, then, been no failures as far as the actual test of the flying capacity of the machine is concerned, for it has never been free in the air at all. The failure of the financial means for continuing these expensive experiments has left the question of their result where it stood before they were undertaken, except that it has been demonstrated that engines can be built, as they have been, of little over one-half the weight that was assigned as the possible minimum by the best builders of France and Germany; that the frame can be made strong enough to carry these engines, and that, so far as any possible prevision can extend, another flight would be successful if the launching were successful; for in this, and in this alone, as far as is known, all the trouble has come.

The experiments have also given necessary information about this launching. They have shown that the method which succeeded perfectly on a smaller scale is insufficient on a larger one, and they have indicated that it is desirable that the launching should take place nearer the surface of the water, either from a track upon the shore or from a house boat large enough to enable the apparatus to be launched at any time with the wings extended and perhaps with wings independent of support from guys. But the construction of this new launching apparatus would involve further considerable expenditures that there are no present means to meet; and this, and this alone, is the cause of their apparent failure.

Failure in the aerodrome itself or its engines there has been none; and it is believed that it is at the moment of success, and when the engineering problems have been solved, that a lack of means has prevented a continuance of the work.

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PRICE, 25 CENTS A NUMBER; $3.00 A YEAR

SCRIBNER'S FOR FEBRUARY

*THIS NUMBER WILL CONTAIN THE BEGINNING OF A NEW FEATURE—**MRS. GEORGE BANCROFT'S LETTERS FROM ENGLAND** WHILE HER HUSBAND WAS MINISTER THERE IN THE IMPORTANT POLITICAL PERIOD OF 1848 AND THE SOCIAL PERIOD OF ROGERS, MACAULAY, MACREADY, LADY MORGAN, AND OTHER NOTABLES.*

¶**CAPTAIN MAHAN'S "WAR OF 1812,"** continued, with an account of the operations just preceding the outbreak.

¶**THE UNDERCURRENT, ROBERT GRANT'S STORY** of love under modern social conditions. Illustrated by F. C. YOHN.

¶**SALVINI, THE GREAT ITALIAN ACTOR,** will be the subject of personal impressions by T. R. Sullivan and Norman Hapgood, who have recently seen him—Mr. Sullivan at the Alfieri Centennial at Asti. Drawings from life by Ettore Tito.

¶**SOME SPANISH GARDENS,** by the author of "A Woman's Hardy Garden," Helena Rutherfurd Ely, with many drawings of beautiful gardens in the Alhambra, the Escorial, etc.

¶**CHARLES KEENE AS AN ETCHER,** an almost unknown phase of his genius as a draughtsman, by M. H. Spielmann, the English art critic, with examples of his etchings, printed in tint.

¶**THREE ILLUSTRATED SHORT STORIES:**

THE KANGAROOS, by MARY MOSS, a humorous story of tennis.

THE CAT AND THE PAINTER, by ELEANOR H. PORTER, an artist's love story.

ON THE HONOR OF A MAN, by W. R LIGHTON, a vigorous Western story.

Poems. *The Point of View.* *The Field of Art.*

THE SCIENTIFIC WORK OF THE GOVERNMENT

By S. P. Langley

Secretary of the Smithsonian Institution

IN the century which saw the birth of this republic, it began to be recognized by thinking men that it is the nature of all things in this universe under like conditions, to act with a uniformity which we call law; that the knowledge of such laws might be found of intimate importance to us, not only individually, but nationally; and that it might be revealed not only in the courses of the stars or by the science of astronomy, but through other sciences yet to be created, and to be founded on the great unifying idea of the Reign of Law.

In the beginning, however, this conception was a vague one, and the scientific bureaus of the Government which to-day embody it, are almost creations of our own times, and science itself has scarcely, apart from its immediate utilities, been universally acknowledged as a fit object of National care.

It emerges into National recognition in the Constitution of the United States itself, which provides that Congress shall have power to promote the progress of science and the useful arts, by securing to authors and inventors the exclusive right to their writings and discoveries. With the exception of the general powers granted under the preamble, no others for the promotion of science were given by the Constitution, and in an age when it was commonly held that the Federal Government possessed only such powers as were expressly granted to it, and that all others were reserved to the States, it is evident that direct encouragement of science could hardly have been expected in the early days of the Republic, and that whatever scientific work was promoted would either be through the Patent Office or in such economic directions as would appear from time to time to be indispensable for the operations of the Government.

It was one of these practical needs which Washington had observed in his military operations, namely, the lack of engineers in the United States, which induced him and others of the military leaders of the Revolution to advocate the establishment of a school at West Point, originally intended more for the creation of an engineer corps than for regular military training, and it was as early as 1783 that the site of this school was determined upon. With its actual establishment we have nothing to do here, except as the fact illustrates how from the very year of the adoption of the Constitution an urgent need apparently necessitated the inauguration by the newly formed Government of a course of scientific instruction.

As might naturally be expected, the first office the Government established which had to do with science, was the Patent Office.

Any attempt to make a survey of the distinctly scientific activities of the Government must necessarily be brief in a series which has already elsewhere considered the numerous incidental agencies for scientific work in bureaus attached to one or another of the Executive Departments. How numerous these are may be inferred from the subjoined list, which is confessedly incomplete, being confined to those bureaus which have a certain number of distinctly scientific employees:

UNDER THE DEPARTMENT OF THE TREASURY:

Supervising Architect's Office,
Director of the Mint,
Light-House Board,
Coast and Geodetic Survey,
Marine Hospital Service,
Bureau of Standards.

DEPARTMENT OF WAR:

Surgeon General's Office,
Chief of Engineers,
Bureau of Ordnance,
Chief Signal Officer.

DEPARTMENT OF THE NAVY:
- Hydrographic Office,
- Naval Observatory,
- Nautical Almanac,
- Bureau of Medicine and Surgery.

DEPARTMENT OF THE INTERIOR:
- Patent Office,
- Geological Survey.

DEPARTMENT OF AGRICULTURE:
- Weather Bureau,
- Bureau of Animal Industry,
- Bureau of Chemistry,
- Office of Experiment Stations,
- Division of Entomology,
- Bureau of Forestry,
- Bureau of Soils,
- Division of Biological Survey,
- Bureau of Plant Industry.

COMMISSION OF FISH AND FISHERIES.

SMITHSONIAN INSTITUTION:
- National Museum,
- Astrophysical Observatory,
- National Zoölogical Park,
- Bureau of American Ethnology.

The newly created DEPARTMENT OF COMMERCE AND LABOR will include, after July 1, 1903, the following before-mentioned bureaus:
- National Bureau of Standards,
- Coast and Geodetic Survey,
- Commission of Fish and Fisheries,
- Light-House Board.

On January 4, 1790, Washington for the first time addressed the Senate and House of Representatives. He urged the "expediency of giving effectual encouragement, as well to the introduction of new and useful inventions from abroad, as to the exertions of skill and genius in producing them at home."

On April 10, 1790, the Patent Office was founded, and it has since that time established a corps of expert examiners in nearly every branch of human ingenuity; it has brought together most valuable records both by way of models and specifications illustrating the development of the mechanic arts in the United States; and without adequate support, without even the use of the funds resulting from its own system; until recently, in cramped quarters, and with an inadequately paid staff, it has still served the Constitution's behest for the development of science and the liberal arts to an extent not equalled by the like service of any other nation.

Were there opportunity to touch on it, it would be interesting to consider the files of the office as a record, not only of human intelligence, but of human ignorance and folly. For perpetual motion machines, there are applications without number, but these are conveniently met by a request for a working model as a necessary preliminary. Even this, however, is not always an assurance, for such devices are proposed as a sextant, which was to be taken down into the hold of the vessel, where an observation made upon a lantern was to give the much-sought-for longitude, the strange thing being that the utility of this invention was vouched for by practical navigators, in one case by a man of known capacity, who asserted that whatever the explanation, as a matter of fact it did do what it professed to do.

Other scientific needs early forced themselves upon the attention of the Government, none more urgently than methods for the improvement of our agricultural system, and these were at the beginning in the care of the Commissioner of Patents. The policy of exploration, both in our Northwest and abroad through the Navy Department, resulted in bringing together models, natural history objects, historical relics, and miscellaneous specimens; and these also were gathered together under the Patent Office, and remained in its custody until the Smithsonian Institution was established, forming, in fact, the nucleus of the present National Museum.

It would appear, then, that apart from certain scientific work carried on under the direction of the various Executive Departments and in strict accord with other needs, the Patent Office was for the first half-century of the Government's existence recognized as its general scientific arm.

Jefferson's interest in science made his administration an especially noteworthy one from a point of view both scientific and educational, and explorations and surveys which ultimately resulted in the establishment both of the Geological Survey and of the Coast Survey, were initiated under his presidency.

The Geological Survey, which was originally a topographic one, practically originated in the expeditions of Lewis, Pike, and Lewis and Clark, but was especially developed by the surveys for a Pacific railway, followed by a long list of explorations which became systematized under King, Hayden, and Wheeler, and definitely organized as the U. S. Geological Survey on March 3, 1879, with Clarence King as its first director, Major J. W. Powell being his immediate successor, and Prof. C. D. Walcott its present incumbent.

The Geological Survey has at present an extensive organization under the Interior Department, devoting its energies to geological investigation of the United States, to topographic surveys conducted on a large scale, to a certain amount of paleontological work growing out of such scientific activities in connection with geological investigations, and, more recently, has had placed under its direction the initiation and carrying out of a vast scheme for the irrigation of the arid regions of the West, which it is expected will add to the fruitful soil of the United States many millions of acres. This Survey is in friendly co-operation with other branches of the Interior Department, notably the Land Office, and with each State in the Union; with the Forestry Bureau of the Department of Agriculture, and with the various State geological and hydrographic surveys, exhibiting a highly intelligent organization of importance to science and of utility to the people, giving to and deriving help from individual geologists connected with many of the large and even small universities and colleges of the country, and presenting altogether the most perfect system of geological investigation, combined with topographic and economic work known to any country. It has produced a body of most capable men who are original in both their economic and scientific work. It has earned the confidence of Congress and the people, and its requirements both for research and publication are being met with a generous hand.

The credit for the inception of the Coast Survey is divided among various persons, though it would appear to have early enlisted the interest of President Jefferson and Secretary Gallatin, and to have been powerfully stimulated by the arrival in this country of Ferdinand Rudolph Hassler, a Swiss, who virtually made the plans upon which the subsequent operations of this Survey were prosecuted.

The Survey dates its origin from an Act of Congress passed in 1807 for surveying the coasts of the United States. It had a checkered and somewhat intermittent career until 1832 when it was reorganized, though its control has from time to time alternated between various departments. It was attached to the Navy Department for a brief period; then for many years it was under the Treasury, and by Act of Congress of this year has been transferred to the new Department of Commerce. It has had as its Superintendents after Hassler such eminent men as Bache, Peirce, Patterson, Hilgard, Mendenhall, Pritchett and its present incumbent, Tittman. It has now over one hundred field officers, and a fleet of twelve steamers and six sailing vessels, besides many launches and small craft. In addition to topographic work it carries on geodetic and magnetic surveys, it has had an office of weights and measures, and has been custodian of the National standards. The development of this last function recently, and with the fullest co-operation of the Coast Survey, has resulted in the establishment of an independent Bureau of Standards of large scope.

Superintendent Tittman, in a recent description of its work, stated that it had since its inception made about 30,000 square miles of topographic surveys, sounded minutely nearly 300,000 square miles of water and made deep-sea soundings over little less than a million square miles. It has completed a first survey of the Atlantic Gulf and Pacific Coasts of the United States, and its triangulations cover between 300,000 and 400,000 square miles. It has published over 500 charts besides the Coast Pilot volumes of the Atlantic and Pacific Coasts; and carefully studied the laws of the earth's magnetism (these latter being now investigated through magnetic observatories in co-operation with foreign governments), and its geodetic work is also being carried on with international co-operation.

The Coast Survey, moreover, is frequently called upon to serve, through its officers as experts, in the determination of boundaries, whether between the States or in matters involving disputes with other nations holding territory adjacent to the United States. Since the Spanish-American War, important labors have devolved upon the Survey in Porto Rico and in the Philippines, where coast surveys are urgently needed and are of high importance for military and commercial purposes.

The difference between the unscientific and the scientific idea of the order of this world, already alluded to, can hardly be emphasized more than in the conception which made a meteorological bureau rational and possible. "The wind bloweth where it listeth" was the conception of ancient times, but the eighteenth century had already reached the idea that the movements of the winds, from the Trades which blew across the planet to the eddy that whirls the dust in the street, are as much subject to law as are the courses of the stars.

A meteorological service, then, was the next one which attracted the attention of the Government. Josiah Meigs, who was in 1814 appointed Commissioner of the General Land Office, was, after Franklin, the most distinguished scientific meteorologist in the United States, and as early as 1817 he urged upon Congress the establishment of meteorological registers in connection with the Land Office. Two years following, through the co-operation of the Surgeon General of the Army, reports were made at the end of each month by the medical officers at the various military posts. The Patent Office, the Engineer Department of the Army, and certain local agencies, furthered this work, while with the discovery of the electro-magnet and the invention of the telegraph, new and immensely important possibilities were opened both in the collection of meteorological data and their use in predicting storms. This procedure, first suggested by W. C. Redfield, was afterwards promoted by Lieutenant Maury, but first realized at the Smithsonian Institution under the immediate supervision of Joseph Henry, who, as early as 1856, had established there the essential features for the prediction of meteorological phenomena. In 1858 a large weather map was on daily exhibition at the Smithsonian Institution, and this work was continued, though necessarily with restricted means, until after the close of the Civil War, when it was formally transferred to the Chief Signal Officer of the Army, where it remained until it was placed under the direction of the Department of Agriculture.

The Weather Bureau is now a highly equipped organization under a Chief, Willis L. Moore, the officer third in rank in the Department. The country is covered with its stations. Its reports, issued twice daily, have come to be looked for in every portion of the United States by all the people, whose daily life is to a certain extent influenced by them, and the value of its work in the saving of life and shipping on the coasts by its prediction of storms and floods, as well as the saving to the crops through timely notice of sudden changes, such as frosts, etc., is incalculable.

The work which the people know best is the general forecasts of the weather, which are conducted on the best obtainable system; forecasts which, though founded on an order of things as subject to law as the courses of the stars, are far from having yet reached the precision of astronomical science, though the results obtained are unrivalled in their excellence by those of any other nation. The preparation of the weather map involves the daily sounding of the heights of the aërial ocean above, simultaneously by observers all over the country, and the joining of these sounding stations on the map by contour lines which indicate the direction of that great aërial ocean's flow. This direction cannot of course be determined with anything like the certainty attainable in the deduction of the path of a star, yet the result, though still a probability only, is a very useful one by which we all guide our daily lives. Will it be greatly better for us if it is ever otherwise, and we come to the time when we know long in advance what the weather will be, and this and many other like uncertainties are wiped out from the variety of our daily life?

These general maps are prepared in the office at Washington, from despatches sent by local offices, and the Bureau's use of the telegraph service alone costs $300,000 per annum. It distributes in the shape of cards, maps, and publications nearly 55,000,000 pieces yearly, and in cases of special agricultural industries, particularly susceptible to destruction through changes in weather, special services have been established, notably for cotton, sugar, and rice, in the Southern States, and for fruit and wheat in California.

Meteorology is a science which, in the main, can only be prosecuted successfully through the Government, owing to the fact that deductions must be based upon a great number of observations carried on for long periods and over wide areas; so that incidentally to, and prerequisite for, the conduct and improvement of its practical and economic work, scientific investigations of the highest moment have been from time to time carried on under the auspices of this branch of the service. The most notable of these in recent years has been the aërial research of studying meteorological phenomena at high altitudes through the use of kites, experiments in wireless telegraphy, and in other fields relating to atmospheric phenomena. A scientific man whose name has long been honorably associated with this original work is Cleveland Abbe, who has been connected with the service since 1867, and who still continues active as a prosecutor of it, and as Editor of the Bureau's publications.

To John Quincy Adams the foundation of the Naval Observatory is due. As early as 1818, as Member of Congress, he proposed it; he urged its establishment while president; after his retirement he continued to favor it. As a Member of the House of Representatives, he endeavored to direct the Smithson bequest toward establishing an astronomical observatory and the publication of a nautical almanac, and he continued his advocacy of such an institution until it was actually formed and placed under the Navy Department.

The beginnings of the Observatory appear actually to have been made during the Presidency of Mr. Adams, for in 1825 there seems to have been the nucleus of an observatory on Capitol Hill. In 1842, Congress authorized the establishment of a permanent depot of charts and instruments which, though not specifically an observatory, was yet, with the approval of President Tyler, actually begun as one, and in 1844 Lieut. M. F. Maury became superintendent of the Depot. In 1861, Gillis became superintendent, and for years the Observatory has been in the general charge of an officer of the navy, with an Astronomical Director and a Superintendent of the Nautical Almanac. Among the prominent scientific officials has been Simon Newcomb, who is ranked among the most distinguished living astronomers. The Observatory's equipment includes ample grounds.

The Observatory, it has been remarked by scientific men not connected with the Government, has an inherent difficulty in its dual organization. It is not unlikely that at some time in the future, its form of organization will be changed; indeed, certain changes have been made looking to the improvement of the present conditions. The present Secretary of the Navy has seriously taken up this question of organization, and useful results may be expected, but it is in fairness to be remembered that the establishments of the Government are created more for the purpose of making the discoveries of science useful to the people, than for original research. It is essential that scientific officers of the Government should have some opportunity to make original studies, if only for the purpose of keeping their minds fresh and open and in touch with the new work of the day; but, in the nature of things, the greater part of the work of the Naval Observatory must be passed in the furthering of utilities which cannot appeal to the popular imagination for applause.

It was in this same period that the beginnings of the National Botanic Garden were laid, though it was not definitely established until much later. The Wilkes Exploring Expedition brought back a large number of living plants, and these were kept for a number of years in the greenhouses adjoining the Patent Office which, as has been above mentioned, temporarily served as the National Department of Science.

In 1852 these collections were removed to the present site of the Botanic Garden on the south side of Pennsylvania Avenue, west of the Capitol, and this Garden has been maintained under the present Superintendent, Mr. W. R. Smith, whose training was had at the Kew Botanical Gardens. It is in no way his fault that the locality and area are not favorable to the extension of the Gardens which, although developed to as high a standard of proficiency as possible under present conditions, can hardly, in their present area and location be expanded into a botanical garden worthy of the Nation, and it is understood that it will be natural that this institution should pass under the charge of some existing department.

Historians and philosophers have not infrequently remarked that the stress of war results in the advancement of science and learning. Napoleon's invasion of Egypt carried in its train the unlocking of the mysteries of the hieroglyphs and the production of the great work "Description de l'Egypte." More recently the foundation of the University of Strassburg signalized the close of the Franco-Prussian War, while the establishment of the Johns Hopkins University was a direct resultant of the war between the States, and was intended, at least in the mind of the founder, to assist in healing the breaches this had created.

It was during the darkest days of this same war that Congress established the National Academy of Sciences, whose creation, foreshadowed by the organization of such private societies as the American Association for the Advancement of Science, had been long in the minds of public men. The actual need of scientific organization was shown during the war, when this nation apparently first awakened to the fact that in every department of activity, and more especially in the military and naval establishments, the services of scientific experts were required. During the war period, Joseph Henry, the Secretary of the Smithsonian Institution, was in almost constant intercourse with President Lincoln; and in that era, before the days of specialization, he was called on to give advice on the most diverse subjects.

It was then that the idea of a non-resident National academy, without localization, like the National Academy of Paris or the Royal Society of London, but composed of eminent men, whose services might be called into requisition by the Government, was created. This body continues in existence, as the most generally representative and dignified aggregation of American men of science, and while suffering under the disadvantages of not having a permanent home, nor officials whose time can be exclusively devoted to its work, has in special cases when called upon, rendered valuable service to the Government by its advice.

The present Department of Agriculture owes its initiative to Henry L. Ellsworth, of Connecticut, who in 1836 began this work by acquiring, without expense, seeds and plants for experimental culture and distributing these under Post-Office franks given him by individual Congressmen.

The first appropriation made by Congress for this purpose was in 1839, when the sum of $1,000 was appropriated from the Patent Office fund to enable the Commissioner to collect and distribute seeds, agricultural statistics, and to make agricultural investigations. It was upon the recommendation of a Commissioner of Patents, David P. Holliday, that Congress established the Bureau of Agriculture with its principal officer as a commissioner, and it continued under this control until 1889, when it became a department of the Government. It was in the same year that the Congress passed a bill introduced by Justin S. Morrill, of Vermont, which was supplemented in 1887 by what was known as the Hatch bill, resulting in the creation or strengthening of State colleges in every State in the Union, the States having the power either to found a new institution or to strengthen an existing one, upon the condition of affording instruction in agriculture and providing a modicum of military training.

The Department of Agriculture has become a large factor in the scientific life of the Government, so large as to render possible in a brief article only the barest enumeration of its activities.

As noted above, it acquired the weather service, which had been successfully carried on through various agencies. In the distribution of seeds already alluded to, its work has grown to vast proportions.

The systematic investigations in nearly every department of biological science directly or remotely connected with the life and health and diseases of animals and plants, the observations of the life habits of all forms which may be either helpful or noxious to agriculture, investigations into the origin and spread, the restriction and the cure of contagious diseases among domestic animals, are but a part of its work. These are carried on in highly equipped laboratories by great numbers of investigators, whose work is welded by excellent organizers into a uniform, compact, and intelligent whole, together with a system of distribution of information of a popular and untechnical character through suitable publications. This is aided by a most efficient support on the part of Congress, and all these and more have been the care of this Department, which has rendered service of incalculable importance, not only in the spread of ascertained knowledge of economic value, but in the enlargement in all domains of such knowledge, presenting the most signal success of such scientific organization yet undertaken by the National Government.

In 1902 a partial reorganization was effected, the most conspicuous advance being the establishment and greater enlargement of the Department of Forestry, which is seriously grappling with the most interesting and important problem of the control of timberlands, not only upon the public reserves, but even in the vast acreage in private hands; and to such work the new division under the present charge of Gifford Pinchot is devoting itself.

The surgeons of the Army and the Navy from early days exhibited their interest in scientific work, a number of them being among the pioneer naturalists and ethnologists in America. As a result of the important professional labors of the surgeons in the Army, and again, as an outgrowth of the War between the States, the Army Medical Museum was established, with the Surgeon-General's Library, which is believed to be the most complete medical library in the world, and which, under the direction of John S. Billings, aided by Robert Fletcher, not only collected a unique library, but issued the most comprehensively arranged and useful catalogue known in any department of learning. It has added to it a medical and surgical collection of the highest importance to the profession and has stimulated the growth within the last few years of a military medical college.

The United States Fish Commission, established in 1871, has illustrated in a most gratifying manner the great possibilities of applying earnest scientific work to the wants of the people, and these have brought about results of vast importance and of great economic value. It was established as an outgrowth of the Smithsonian Institution under its Secretary, Spencer F. Baird, who is credited with the statement that a mile of ocean along our coasts can furnish more food products than ten miles of fertile land. During his lifetime it was to a certain extent carried on in connection with the Smithsonian Institution, and has done a great work for the advancement of our knowledge of the life of every description of creature inhabiting the fresh waters of our country and the oceans surrounding it. It has increased to a degree hardly to be believed the quantity of fish available for our people, and has put within the reach of the poor, healthful and nourishing food, at one time only possible for the rich to enjoy.

The general work of the Commission as administered is under three divisions, which are known as:

(1) Division of Inquiry respecting food fishes.

(2) Division of Fisheries.

(3) Division of Fish-Culture.

The principal part of its scientific work is under the first division, ordinarily known as the Division of Scientific Inquiry, and comprises:

1. The investigation of the fishing-grounds of the Atlantic, Gulf, and Pacific Coasts and the inland waters of the United States, with the view of determining the food resources and the developing of the commercial fisheries.

2. The investigation of the causes of the decrease of food fishes in the waters of the United States.

3. The study of the waters of the coast and interior to determine the feasibility of increasing their natural resources.

4. The dissemination of information concerning the distribution and habits of marine animals and their capture, and their preparation for the markets.

5. Examination into the adaptability of sites for fish-cultural stations and investigation of the diseases incident to fishes at such stations and at large.

The second division, known as the Division of Fisheries, deals with the economic phases of the fisheries themselves, such as the collection of statistical data, the study of the apparatus and methods of capture with special reference to their utility and their effect on the fisheries, the best methods of utilizing the products, the effect of fishery legislation, international fishery relations and all other matters affecting the economy of aquatic resources.

The work of the Division of Fish Culture consists in the hatching and distribution of marine and fresh-water fishes for the purpose of maintaining existing fisheries, restocking grounds that have been depleted by over-fishing or injurious methods, and creating new fisheries either by the introduction of foreign fishes in the waters of the United States or transplanting native fishes as, for example, the establishment of the shad and striped bass fishery on the Pacific Coast. The results of the work of this division have been most gratifying. Millions of pounds of fish are now captured in waters where they were originally unknown, and equally valuable results have been secured not only in maintaining the various important fisheries of the Atlantic and Pacific Coasts, but grounds which had become depleted are now supporting valuable fisheries. This work is carried on at thirty-five hatching stations established by Act of Congress in the various States, and four railroad cars are in constant use in distributing their products, besides a number of small boats, launches, and vessels.

Entirely new avenues of scientific research have been opened by the Commission, with which the name of G. Brown Goode will ever be associated, most notably in the discovery of the deep-sea forms of the North Atlantic basin. His work, with the co-operation of such men of distinction as Alexander Agassiz and David Starr Jordan, aided by the faithful and efficient labors of a large staff of ichthyologists and fish-culturists, has resulted in securing a systematic investigation of the waters of the United States, and the biological and physical problems which they present. By a study of the methods of fisheries past and present, the causes of deterioration of fish in various waters have been discovered and remedies applied, and useful food fishes have been enormously multiplied throughout the country; whilst important international problems dealing both with sea and fresh-water fisheries, and with the problem of the fur seals, have been powerfully aided by this Commission and by the experts connected with it.

And yet the expense of the Commission is inconsiderable when compared with the increase of wealth and the means of livelihood it affords the American people.

There is practically no department of the Government in which expert scientific work is not called for. Especially is this true of the medical arms of the Army and of the Navy. The engineering work of both these services, the ordnance work, the construction of vessels, the Bureau of Steam Engineering, the Bureau of Navigation, all represent an aggregation of men making direct application of science to the needs of the Government, not infrequently resulting in original contributions to knowledge.

Among these bureaus the Hydrographic Office of the Navy is deserving of special mention as illustrating an undertaking where the most direct applications of science are made. The hydrographic charts, over a thouand in number, plotted from original naval surveys, some made directly by the Navy, some by vessels under the supervision of the Coast Survey, the deep-sea soundings, the issuing of sailing directions and of other aids to navigation, and the establishment of signals for derelicts are but a few instances of the work of this office.

It is not so very long, as nations count years, since the length of a king of England's arm, marked rudely on an iron bar by a blacksmith's chisel, was made a national standard of lengths; and this was a real advance over a condition of things existing when almost every country had its own measures.

In contrast to this, we have now in Washington the Bureau of Standards, alluded to above in connection with the Coast Survey, which is intended for the purpose of the standardization of machines for measurement and other service, together with the instruments used in everyday life, as well as for philosophical apparatus. It is under the charge of a capable physicist and administrator, S. W. Stratton. Its work will comprehend researches in the domain of physics, extending both into chemistry and engineering, and Congress has appropriated funds for the erection of buildings and the purchase of apparatus. A mechanical laboratory costing about $125,000 and a physical building costing about $200,000 will be erected. This bureau is so new that its results can hardly yet be spoken of, but in a country like ours, in which so much of the national wealth and progress is due to inventive genius and improvement of machinery, any steps tending to the further introduction of exactness in this important branch of our national life cannot fail to be productive of most useful results.

The Marine Hospital Service and the Department of Public Health, which among other things has under its care all federal supervision of these departments of sanitation, and so far as possible without conflicting with state laws, the control of all persons suffering from contagious diseases, has rendered a great service to the country by its intelligent handling of the various contagious diseases and plagues which from time to time have invaded our shores. Under this service a laboratory has been recently established where constant and successful experiments are being made in that most important branch of medical science, preventive medicine.

The Bureau of Labor, established in 1885, has, incidentally to its practical functions, done much valuable research work and in the collection and publication of statistics bearing upon social and economic problems, and has been effective in bringing about a better understanding of the conditions of human labor.

Many agencies remain unspoken of, but among these, two, the National Library, known to the law as the Library of Congress, and the Smithsonian Institution, must have separate mention.

Scientific research in the modern sense is impracticable without access to books, since it is incumbent upon each investigator to examine the works of his predecessors, and in announcing his results to state the extent of his dependence upon those who have preceded him in the field.

The Library of Congress was not established, of course, with any such purpose in view. It was at first strictly what its name implies, and its need was felt even as early as the Continental Congress. The origin of the present library goes back to the removal of the Capitol to Washington, but in the very beginning, as early as 1806, it was recognized that Congress itself required something different from mere law books or important books of reference, and the general subjects which might now be termed polite literature and "the humanities" began to be incorporated in the Library at that early date. This policy, which has been kept up and extended, has made the Library in fact, if not in name, a National Library.

From the Capitol it was removed in 1897 to its splendid new building, one certainly better adapted to library purposes than any other in the world. This Library contained, in 1902, nearly 800,000 books and over 300,000 pamphlets, a total of over 1,000,000 titles, this being exclusive of the law library and manuscripts, maps, pieces of music, and prints, which together make a total considerably over half a million. It is now virtually a general library, and while it bears some special relation to the needs of Congress, and of necessity devotes itself as one of its main features to Americana, is still a library universal in scope, and in it there is represented every department of human knowledge. Upon it the men of science connected with the Government may draw freely.

By judicious expenditure of its enlarged means for the purchase of books within recent years, it has added materially to its collections in the physical and natural sciences, It is engaged in the publication of special lists and bibliographies which, while not directly intended for scientific purposes, yet have much value to science, and are a most efficient aid to the prosecution of the scientific work of every branch of the Government.

Of necessity special libraries also exist; that of the Surgeon-General's Office, the most notable, has already been alluded to, but the others, maintained by the Geological Survey, the Department of Agriculture, and indeed in every scientific bureau and office of the Government, are of increasing value and utility to persons engaged in special work. These are conducted and added to in harmonious relations with each other, and with the Library of Congress; so that without a common organization, there has grown up an understanding which avoids unnecessary duplication, and which arranges for the interchange of books among the various libraries, and altogether furnishes a most efficient system for procuring and using scientific works.

I come last to speak of the Smithsonian Institution. I have endeavored, in the little space allotted, to briefly review the scientific activities of the United States Government. Every administrator is prone to the natural risk of magnifying the work of his own department, but the Smithsonian Institution, occupying a unique position in that it is a kind of ward of the Nation, has secured for itself so firm a hold upon the interest of the people of this country, and so distinguished a position abroad, that it may be spoken of objectively.

The Smithsonian Institution, as is known to all men, originated in the bequest of an Englishmen, James Smithson, who died in 1829 and left his fortune to the United States to found at Washington an establishment under the name of the Smithsonian Institution" for the increase and diffusion of knowledge among men." Congress after much discussion passed a law in 1846 founding the Institution. It created the "Establishment," or corporate body, consisting of the President of the United States, the Vice-President, the Chief Justice and the members of the Cabinet. It provided for a Board of Regents for its government, and for a Secretary who, as Secretary to both of these Boards, should be the executive officer. It named as the principal purposes to which the Institution was to be devoted, the establishment of a library, of a museum, of a gallery of art, the giving of lectures, and other cognate methods in carrying out the will of its founder.

At the time of its organization, the Institution was relatively the best endowed scientific establishment in America. Its various purposes enumerated in its charter have been carried out. It formed a library (now to a great extent deposited in the Library of Congress) which is the best collection of transactions of learned societies and of scientific periodicals in the United States, and one of the great collections of the world. It began a museum, now known as the National Museum, and still under its charge, which in everything that pertains to the fauna and flora, the ethnology and geology of North America, is the most considerable in existence, and which bears within itself the nuclei of most important collections in American History in the progress of mechanic arts and in all the departments of learning which go to make up a museum of universal scope.

The Institution exists for two main purposes:

1. The Increase, and
2. The Diffusion, of Knowledge.

In addition to carrying on the objects in furtherance of these purposes enjoined upon it by its fundamental law, it has published from its private fund contributions for the increase of human knowledge of almost every description, resulting from explorations, the study of collections, original investigations, and experiment.

It has established also a system of international scientific exchanges which has become a recognized means of bringing the learned institutions and learned men of all countries into closer relations.

The income of this original fund has been in later years supplemented by annual appropriations from the Government, for extending and carrying on the work of the Museum, the Exchanges, the maintenance of a Zoölogical Park, an Astrophysical Observatory, and a Bureau of American Ethnology. The relation of the Institution, as such, to these various agencies, is that of a trustee for the National Government, entrusted with their direction and supervision, and bearing the responsibility for their proper and effective administration.

The National Museum, under the direction of the Smithsonian Institution, does not consist solely of objects for entertainment, but is rather a vast organized collection of the ideas and works of man on this continent, beginning with primitive man, and showing how his simple arts and his simple faith grew into complex culture and organized religions. It is impossible here to give an adequate conception of the range of this collection, which includes with the material products of this continent, relics of the Nation's history in war and peace, and perhaps the finest existing collection of personal relics of Washington and other historic Americans. It is the place of deposit of the collections of the Bureau of Ethnology, which, under the care of the late Major Powell, has described and published the history of primitive American man. Congress has just appropriated three and one-half millions of dollars for the adequate housing of these great collections.

The Bureau of International Exchanges is rather for the convenience of scientific men in the matter of diffusing their researches than a work of science in itself, though it spreads its operations over the whole world and has 30,000 correspondents outside of the United States.

The National Zoölogical Park grew from a small collection maintained in the rear of the present building of the Institution to the occupancy of its present picturesque grounds of 167 acres, now embraced in the ever-growing city. Its fundamental object is the preservation of our North American game. It is not supposed that it can in this small space alone keep from extinction the races which are fast following the buffalo, but it can offer a city of refuge for them and an object-lesson under the immediate eye of Congress.

The Astrophysical Observatory is dealing with man's relations to the sun and with problems which affect his welfare in a material sense. It has extended the known spectrum, through the invisible infra-red, to an extent many times that known to Sir Isaac Newton.

The establishment of the Smithsonian Institution at the time when it came into existence was a matter of supreme importance for the development of science in America. Sixty years ago, the funds for research were small and the avenues of publication inconsiderable. Two or three important scientific societies were in existence, but their funds were limited. No body of scientific men anywhere acknowledged a leader, and at a time, too, when most important investigations both in the physical and natural sciences were being made.

The acceptance by the Government of the trusteeship of this fund of Smithson's gave a national centre for American science to gather about. It brought into existence, too, an organization which in Joseph Henry found a man strong enough to take up uninvestigated problems which had not yet been moulded into definite practical shape, and to advance their solution to a point where others might avail themselves of the Institution's work.

This, in brief, was the early policy of the Institution, and continues so to the present day. As indicated in the preceding portions of this article, the Smithsonian Institution has had much to do with originating work in other Government scientific departments. The importance of its early contributions to meteorology and to the establishment of what is now the weather service is universally acknowledged. It gave aid to those explorations which in a large measure resulted in the formation of the Geological Survey. The Fish Commission, the Bureau of Ethnology and the National Herbarium originated here. The system of international exchange of scientific publications, projected by the Smithsonian, found no one to take it up until the Institution organized its work, and this system both of Governmental exchanges and scientific exchanges, it continues to administer. Not so well known are its relations to such remote matters as the acoustics of the Hall of the House of Representatives, the methods by which vessels signal in fogs and the work of the Light-House Board.

In pursuance of its motto "Per Orbem," it has aided by grants not only in the United States but in other parts of the world, investigators engaged in original scientific work. It has published treatises containing new information of great value to students, and it has distributed among the people probably more than a half million volumes containing accurate scientific information in popular form. Ethnological researches among the American Indians were powerfully stimulated by it from its inception, and the first volume of its well-known series of "Smithsonian Contributions to Knowledge" was upon this subject. The researches made here in connection with the problem of aërial navigation have been largely instrumental in taking this work, heretofore derided and considered impracticable, into the realms of respectable and active scientific investigation.

Through a special fund, known as the Hodgkins Fund, a portion of which is devoted to the investigation of atmospheric air in relation to the welfare of man, our knowledge of the composition and properties of the atmosphere has been greatly extended. The Institution laid the foundation of methods of scientific library work in cataloguing, which so distinguishes American libraries from others, it originated the project of cataloguing all scientific papers by international co-operation and is at present, in default of any action by Congress, acting as the representative of the United States in the present International Catalogue of Scientific Literature published by a bureau with its seat at London. Under its auspices, and through it, such organizations as the American Historical Association have issued many works of value to historians and public men.

It remains in conclusion, to point out the fact not usually recognized, that it was through the gift of Smithson and its acceptance by the Government that the steps for Governmental science, which were deemed difficult under constitutional limitations, were gradually made easy after the Institution was founded. Direct appropriations for science were, and continue to be, resisted upon constitutional grounds, but when the Smithson Fund was finally accepted by the Government and provision for carrying out the will of its founder was made, and Congress imposed upon the Institution obligations which its fund was not sufficient to meet, notably the establishment of a museum for the reception, care, and exhibition of the results of Government exploring expeditions, the step was made easy for Congress to provide through the Institution for carrying out its own behests; and much easier than if the different organization of such establishments outright and upon an independent basis had been attempted.

It is difficult to get practical men to provide for projects which are still in the experimental stage. The work necessary to the creation of the Weather Bureau and the Fish Commission would probably never have been accomplished but for the existence of an agency which provided for the initial and experimental stages of these two important National scientific projects. Indeed, so obviously practical a thing as scientific agriculture was promoted by the Smithsonian Institution in conjunction with the Commissioner of Patents, in the days when Congress had not yet seen its way clear to take this up.

It is not to be doubted that the philosophical workers of the United States will in the future recognize as the two dominant factors which produced the National scientific activity, first, the practical need of the Government for expert work in every department, and second, the establishment of the Smithsonian Institution, which, without violating the political consciences of our statesmen, enabled them to provide the means for scientific work whose ultimate economic importance has proven of the highest value to the Nation.

Such and so numerous are the scientific bureaus of the Government that it has seemed impossible, in this brief space, to do more than catalogue them, though each would become an interesting study if treated in detail, which would occupy a volume rather than an article.

A METHOD OF AVOIDING PERSONAL EQUATION IN TRANSIT OBSERVATIONS

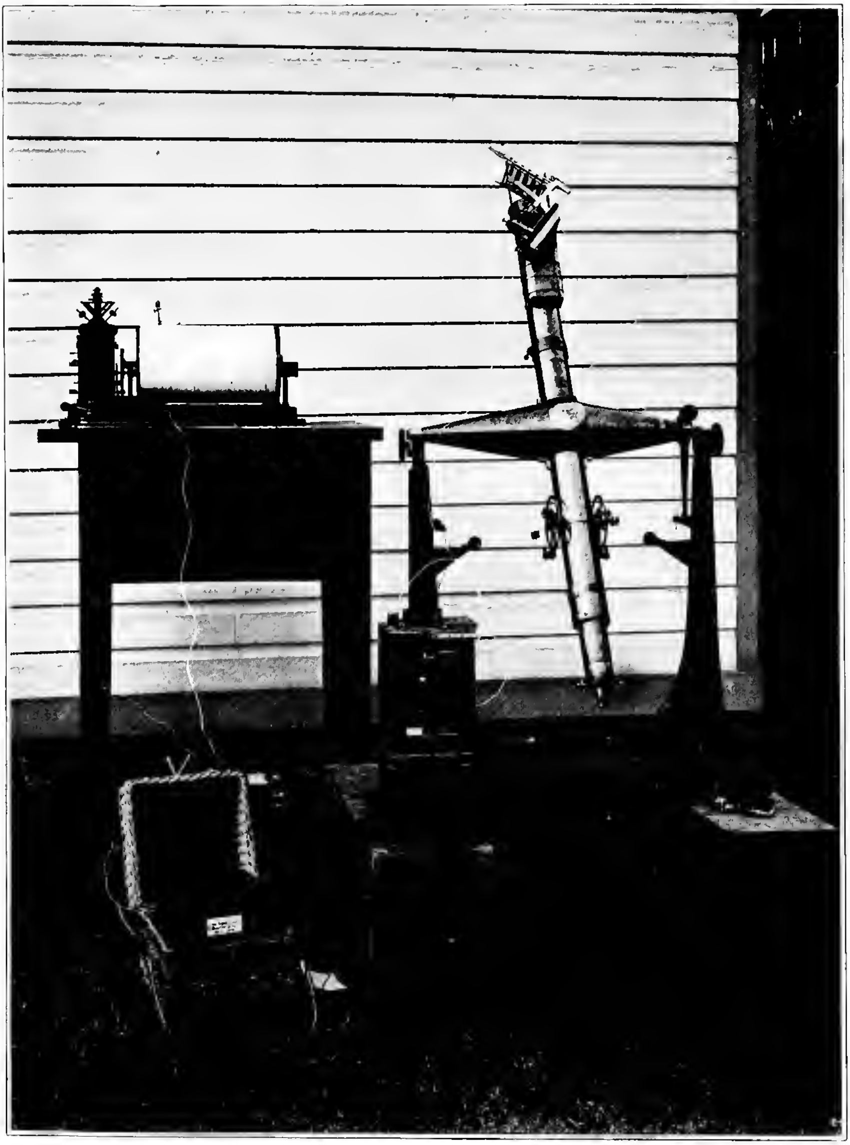
BY

S. P. LANGLEY

Reprinted from Smithsonian Miscellaneous Collections
(Quarterly Issue), Volume 45

No. 1446

City of Washington
Published by the Smithsonian Institution
April 11, 1904



THE PERSONAL EQUATION MACHINE, TRANSIT, CHRONOMETER, AND CHRONOGRAPH.

Reprinted from SMITHSONIAN MISCELLANEOUS COLLECTIONS (QUARTERLY ISSUE),
volume 45. Published April 11, 1904

A METHOD OF AVOIDING PERSONAL EQUATION IN TRANSIT OBSERVATIONS

By S. P. LANGLEY

Many years ago the writer devised and published[1] preliminary accounts of a method for avoiding the so-called "personal equation" in observations of the time of transit of stars. The following quotation from the article cited will give the fundamental idea described in the former article, to which the present one adds some more recent experiments and slight modifications. The writer, after speaking of the comparative merits of and objections to a photographic method, and one where the star is seen intermittently projected on wires which are illuminated by consecutive flashes, observes:

> "There is one particular case, however, where the result is the same for both, that, namely, where when the flash comes, the star is on the wire and bisected by it; in this case we know its position as accurately by the eye, as if we bisected its image on the plate by the wire of our micrometer. If we suppose, then, that by a happy accident, the flash came just as the star was crossing the first wire, this wire would be sharply defined on the disc of the star and bisecting it, and a simultaneous record on the chronograph (made without the intervention of the observer) would evidently give us the same result as though the star had recorded its own passage by an electrical contact. Further, we may particularly notice that it is immaterial whether the star was at rest, when thus seen, or in motion. Now what we have just supposed as a single case of a favorable chance among hundreds, it will be our task to *make* occur, whatever the wire interval, and for any star observed."

In illustration let it be imagined that the observer is watching the passage of the star through the field of his telescope, but that he has no illumination of that field sufficient to discern the wires of the tally. Suddenly a flash illuminates the field, and we may suppose that by accident this occurs just at the instant when the star image is bisected by the central wire. If now the flash had recorded itself automatically on the chronograph, an observation independent of the ordinary personal equation would have been obtained, since we know where the star was at that recorded time of observation.

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[1] *American Journal of Science and Arts,* July, 1877, pages 55–60.

The device I have already described consisted in making this unlikely accident occur for any star, and for numerous wires of the tally.

At the time when it was first devised, such a method consisting not in correcting time observations for personal error committed, but in preventing the committal of such errors, was unique, and it had for the writer the good fortune of attracting the interest and commendation of Professor Clerk Maxwell. But the writer was prevented by distinct duties from developing it, and more recently Repsold and others have attacked the same problem in another way, with much success. Still it is possible that this older method may prove of value, and I have thought best to give this brief account of some recent trials of it.

The instrument whose purpose is to illuminate the field at the required instants, was constructed from my design by A. Hilger, as follows: A conical pendulum consisting of a graduated rod with a heavy ball which may be set at any height on the rod, is suspended by two pairs of thin flat springs acting as a universal joint. At the lower end of the rod is a needle which, as the pendulum revolves, governs the rotation of a grooved arm carried by a clock-work which also drives a drum carrying fine platinum brushes which make instantaneous electrical contacts at each revolution. (Plate LVII.) Thus the intervals between these contacts are governed by the rate of the conical pendulum, which itself goes fast or slow according as the ball is raised or lowered on the rod, this rod being graduated so as to correspond with the declination of any star between 0° and 60°. Knowing the declination of the star, it can be arranged beforehand, therefore, that the interval between contacts is equal to the average interval between transits of the observed star across the successive wires of the tally. By appropriate electrical connections this system of contacts is caused to produce a system of instantaneous discharges in a vacuum tube, which is used in place of a lamp to illuminate the cross wires of the telescope, and if the star be first seen, for instance, half way between the first and second wires of the tally, it will be seen at the second illumination half way between the second and third wires, and so on.

We have thus arranged that flashes shall occur at recorded instants, without limit as to number, and at intervals such that if one, by chance, occurs when the star is bisected by a wire, it will be seen bisected by all the wires in succession. It now remains to arrange that this bisection shall actually occur. This is accomplished by providing an independent control of the position of the contact points under the clockwork, so that with a cord in the hand of the observer he can cause the series of contacts to occur sooner or later, without altering the interval regulated by the pendulum. Thus if the flash comes a little too early for bisection of the star image by the first wire of the tally, a slight adjustment is made, delaying the succeeding flashes, and with a little practice one or two adjustments suffice to secure bisection. To add the weight of independent observations, several displacements and readjustments may be made during the passage of the star over the tally.

As the times of all the flashes are recorded automatically upon the chronograph, an independent signal is made by the observer to mark each flash which has revealed a satisfactory bisection.

I have thus far given the device substantially as described in the early paper. To test the value of the method under circumstances admitting of distinguishing error, an artificial star was arranged to move by an accurate clockwork at about the apparent rate of an actual equatorial star. The shaft which carried the screw by means of which the artificial star was moved, had upon it an arm provided with an adjustable point which instantaneously broke an electrical circuit at each revolution. By careful adjustment, *with the artificial star stationary,* it was arranged that exact bisection took place at the middle wire of the tally, and with the point in position to break contact. Thus the artificial star was caused to record its own time of transit upon the chronograph wholly without personal equation of time.

A transit instrument of 2¾ inches aperture and 48 inches focal length was kindly loaned me by the Superintendent of the United States Coast and Geodetic Survey, for the purpose of making an experimental test of the personal equation machine.

In reconsidering the theory of the method a possible cause of failure appeared. The observer sees the star continually moving across the limited field of view, and thus his mind may unconsciously be influenced by the perception of motion, just as it undoubtedly is in observations by the usual method. In order to

TEST OF PERSONAL EQUATION MACHINE USED BY IMPROVED METHOD.

	Observer, C. G. A.				Observer, F. E. F.				Observer, N. E. G.			
Method	Usual Method Without Personal Equation Machine		With Personal Equation Machine Intermittently Obscuring Star		Usual Method Without Personal Equation Machine		With Personal Equation Machine Intermittently Obscuring Star		Usual Method Without Personal Equation Machine		With Personal Equation Machine Intermittently Obscuring Star	
	Error of Observed Time of Transit	Deviation From Mean Error	Error of Observed Time of Transit	Deviation From Mean Error	Error of Observed Time of Transit	Deviation From Mean Error	Error of Observed Time of Transit	Deviation From Mean Error	Error of Observed Time of Transit	Deviation From Mean Error	Error of Observed Time of Transit	Deviation From Mean Error
Date	July 25, 1902		July 25, 1902		July 25, 1902		July 25, 1902		July 25, 1902		July 25, 1902	
1	—0^{s}.14	0^{s}.034	—0^{s}.02	0^{s}.004	—0^{s}.02	0^{s}.002	+0^{s}.03	0^{s}.08	+0^{s}.02	0^{s}.086	—0^{s}.004	0^{s}.033
2	— .20	.094	+ .05	.066	— .09	.068	— .02	.03	.06	.046	+ .040	.011
3	— .14	.034	.01	.026	— .10	.078	— .07	.02	.04	.066	— .056	.085
4	— .06	.046	.07	.086	+ .01	.032	+ .06	.11	.20	.094	+ .008	.021
5	— .05	.056	.00	.016	— .08	.058	± .00	.05	.00	.106	+ .007	.022
6	— .12	.014	— .06	.044	— .03	.008	— .03	.08	.18	.074	+ .002	.027
7	— .18	.074	+ .05	.034	— .05	.038	— .10	.05	.03	.076	— .012	.041
8	— .09	.016	— .03	.014	+ .06	.082	— .08	.03	.05	.056	+ .054	.025
9	— .20	.094	+ .06	.076	— .02	.002	— .08	.03	.16	.054	— .007	.036
10	— .13	.024	— .10	.084	— .14	.118	— .09	.04	.25	.144	+ .120	.091
11	+ .01	.116	— .12	.104	— .03	.052	— .08	.03	.26	.154	+ .058	.029
12	— .01	.096	— .08	.064	+ .06	.082	— .12	.07	.10	.006	+ .085	.056
13	— .06	.046			+ .09	.112	— .13	.08	.03	.076	+ .049	.020
14	— .04	.066			— .03	.008	— .07	.02			+ .030	.001
15	— .12	.014					— .02	.03			+ .063	.034
16	— .17	.064									+ .027	.002
Mean.	—0.106	0.055	—0.016	0.052	—0.022	0.053	—0.050	0.050	+0.106	0.080	+0.029	0.033

overcome this objection a rearrangement of the apparatus was made so that instead of intermittently obscuring the wires of the tally, the wires were now continuously illuminated as in the usual method, but the star itself was obscured by a shutter, except at the instants when the wires had formerly been illuminated by the glow tube. As thus modified the observer by this second method sees the star only during well-separated intervals of a hundredth of a second or less, so that all appearance of motion is avoided. This device, suggested by Mr. Abbot, appears to be an improvement on the former method, and has been used in the observations presented in the accompanying table. These observations were made by three observers, C. G. A., F. E. F., and N. E. G., of whom F. E. F. was most experienced in observing transits without the personal equation machine, and accustomed to its use by the former method, but less practiced than the others with the machine as now employed.

The general result appears to be in favor of the personal equation machine. In the case of the observer C. G. A., whose usual equation was about —0.10 seconds when observing without the machine, the error became only —0.016 seconds with the machine; and the average deviation was reduced slightly. Similarly with the observer N. E. G., whose direct observations were usually rather variable, but which now yielded an average personal equation of +0.106 seconds, the employment of the machine was of marked advantage, reducing his personal equation from 0.106 to 0.029 seconds and the average deviation of it from 0.080 to 0.033 seconds. With the observer F. E. F., the usual personal equation was small and the average deviation of it was slightly reduced.

So far as it has been tried, then, the advantage of observing by the aid of this device seems to be marked.

THE REMOVAL OF THE REMAINS OF JAMES SMITHSON

BY

S. P. LANGLEY

Reprinted from SMITHSONIAN MISCELLANEOUS COLLECTIONS
(QUARTERLY ISSUE), Volume 45

No. 1449

City of Washington
Published by the Smithsonian Institution
April 11, 1904

Reprinted from SMITHSONIAN MISCELLANEOUS COLLECTIONS (QUARTERLY ISSUE), volume 45. Published April 11, 1904.

James Smithson

THE REMOVAL OF THE REMAINS OF JAMES SMITHSON

BY S. P. LANGLEY

The remains of James Smithson, founder of the Smithsonian Institution, who died June 27, 1829, at Genoa, Italy, were deposited in the little cemetery belonging to the English Church, on the Heights of San Benigno, a solitary spot planted with cypress trees, and looking down upon the Gulf of Genoa. In 1891 the Secretary of the Institution visited the grave, and, with the approval of the Regents, deposited with the Secretary of the English Church Fund a small sum to invest in Italian five-percent rentes, for its perpetual care. It was visited on two later occasions by the Secretary, who placed a bronze tablet containing a bas-relief of Smithson, in the English Church, and one also at the tomb, whence it was subsequently stolen.

At this time it was understood that there was a probability that before many years the site of the cemetery might be required by the Italian authorities, and the following communication to this effect was made to the Secretary on the 24th of November, 1900, by the Committee of the British Burial Ground:

7 VIA GARIBALDI, GENOA,
24 November, 1900.

SAMUEL PIERPONT LANGLEY, Esq., LL.D., D.C.L.,
Smithsonian Institution, Washington.

Dear Sir:

The Committee of the British Burial Ground of Genoa (of which you are aware Her Majesty's Consul is Chairman), fully realizing how keenly you are interested in all that concerns the resting place of the respected Founder of your Institution, has deputed me to write to you and lay before you the present position of our Cemetery.

It will lie in your recollection that when I accompanied you some years ago up to the heights of San Benigno, you were struck by the enormous quarry which was slowly but surely eating its way towards us from the sea through the rocky side of the hill on which we stand, and excavation has lately come so close to us that the intervention of the Consul became necessary to arrest further advance on the plea that our property would be endangered if the quarrying were carried on.

Actual blasting has in fact been put an end to for the present, and the Cemetery (although the boundary wall is now on the very edge of the excavation) remains untouched; but the local authorities who are the owners of the quarry have given us to understand that they need more stone for their harbor works, and are therefore anxious to see our graves transferred from the position they now occupy, for which purpose they would give us a suitable piece of ground in another part of the town and would also undertake the due and fitting transport of the remains. Should our answer be in the negative, it is intimated to us that in five years' time, in 1905, the term for applying the Law for Public Utility (twenty years after the date of the last burial) will have been reached, and we shall then have to give up of necessity what we are now asked to yield as a concession.

Under the circumstances, the Committee have decided that it is their best policy, in the interest of all concerned, to begin to negotiate at once for the transfer on a decorous footing of the British Cemetery and all its tombs, and although some considerable time may elapse before this transfer is accomplished, yet it is evident that the time has now come for us to ask you to prepare your decision as to what is to be done with regard to the James Smithson remains. Are they to be laid with all possible care and reverence in new ground here, or are they to be conveyed to the United States?

Awaiting the pleasure of your reply, I beg to remain,

Very faithfully yours,
E. A. LE MESURIER.

This communication was laid before the Regents, who, at their meeting of January 23, 1901, adopted the following resolution:

RESOLVED: In view of the proposed abolition of the English Cemetery at Genoa which contains the remains of James Smithson, that the Secretary be requested to arrange either with the English Church or with the authorities of the National Burying Ground at Genoa for the re-interment of Smithson's remains, and the transfer of the original monument.

At a meeting of the Regents held on the 22d of January, 1902, the Secretary recalled to the Board the resolution adopted at its previous meeting, and stated that the wishes of the Board with regard to the removal of the remains of Smithson had been communicated to Mr. E. A. Le Mesurier, one of the officers of the English Church at Genoa, who, under date of December 23, 1901, had replied in part as follows:

> You are aware that our hope is eventually to obtain for our countrymen a separate burying-place which by an easy, and I may say obvious, arrangement might be made to give shelter not merely to British subjects but to American also. I regret to say, however, that I see no chance for the present of this most desirable consummation, as the authorities (apparently in consequence of the difficulty of finding an alternative site) have withdrawn their offer of providing us with a fresh cemetery if we allowed them to transfer at once all remains from San Benigno, where your Founder rests. The present policy of the authorities is presumably to let things remain as they are until the time comes (three years or so hence) when the Law of Public Utility will strengthen their hands as to taking possession of the San Benigno ground, of course under the obligation of transporting the remains elsewhere, which would in all probability mean a portion of the general Protestant cemetery and not a separate place of interment. When the time for the transfer approaches, it will be obviously expedient to apply to the British Ambassador at Rome (backed up, as we are confident will be the case, by the friendly offices of the Representative of the United States) to put the case before the Italian Government, so that the local authorities may be enjoined to carry out the process with all due reverence, and if possible (as it ought to be possible) to a especially reserved new Cemetery. Our Consul is most fully alive to the importance of diplomatic support and will take the initiative in due course.

Doctor A. Graham Bell, a member of the Board, took occasion to reiterate the strong feeling expressed by him at the preceding meeting of the Regents, that the remains of Smithson should be brought to this country.

At the regular meeting of the Regents held on December 8, 1903, the Secretary read the following letter from the United States Consul at Genoa, and that from the Committee of the British Burial Ground Fund in Genoa:

CONSULAR SERVICE, U. S. A.
GENOA, ITALY, November 24th, 1903.

DR. S. P. LANGLEY,
Secretary of the Smithsonian Institution,
Washington, D. C.

Dear Sir:

Referring to my letter of June 30th last, I now forward enclosed circular letter just received, which shows that the final step in the demolition of the old British Cemetery is about to be taken. If you have any desires to express as to the disposition of the remains of James Smithson, and instructions to give, these should be made known with as little delay as possible.

You will see by my letter above mentioned that there is a tradition here of some opposition to the removal of James Smithson's body to America on the part of relatives in Europe. I was informed at the British Consulate that the source and character of this opposition has been made known at some time to the Smithsonian Institution. I can learn nothing further of it here. You are therefore in possession of such information as can be had for your guidance in adopting a course of action as to the disposition of the remains. You will notice that instructions are requested by the Cemetery Committee before January 1st, 1904.

I am quite at your service in the matter of any assistance that may be needed here.

I am informed by the Agent of the American Express Company here, he would transport the body to Washington for $203. This would not include taking up the body nor the coffin, the boxing, etc. A Government tax of 360 lire (approximately $72.00) has also to be paid on every body taken out of the country. The total expense at a rough estimate might come to $400.00 or to $500.00 if something unusually handsome in the way of a casket were called for. All due economy would be used.

Believe me

Very truly yours,

WILLIAM HENRY BISHOP,
U. S. Consul.

(*Enclosure*)

GENOA, November 23, 1903.

Sir,

We have the honor to inform you that the old British Cemetery, on the heights of San Benigno in this city, has been expropriated by the Italian authorities, and will shortly be demolished.

The remains of all persons buried there will be removed to the new British Cemetery at public expense, and the tombstones will also be removed, and re-erected over the new graves, by the undersigned Committee, unless otherwise desired by the representatives of the deceased.

It has been impossible to ascertain the addresses of these representatives in every case, and this letter is sent to you with reference to the grave of James Smithson.

Kindly address any communication you may wish to make on this matter to

NOEL LEES Esq.,
Care of H. B. M.'s Consul-General—Genoa
before January 1st, 1904.

And believe us,
Yours faithfully

THE COMMITTEE
BRITISH BURIAL GROUND FUND
Genoa.

Doctor Bell renewed the proposal made by him at the previous meeting of the Regents, that the remains of Smithson be brought to this country at his expense, and after some remarks the following resolutions were adopted:

RESOLVED: That Doctor A. Graham Bell be appointed as a committee to take charge of the matter of the removal of the remains of James Smithson from Genoa to Washington, with the request that the negotiations and removal be conducted quietly and privately.

RESOLVED: That upon the conclusion of this duty, all expenses involved by it be reimbursed to Doctor Bell from the funds of the Institution.

Doctor Bell, accompanied by Mrs. Bell, sailed on the 15th of December for the port of Cherbourg in France, and going thence to Genoa, commenced at once the arrangements for the transfer of the remains, arrangements which would have occupied a quite indefinite time and incurred a corresponding delay, except for the aid given by the United States Consul, Mr. William Henry Bishop, which Doctor Bell gladly acknowledges.

On opening the tomb in the presence of Mr. Bell, the United States Consul, Noel Lees, Esq. (official representative of the British Burial Ground Fund Association), and other witnesses, it was found that the remains of Smithson, represented by the skeleton, were in fair preservation, although the wooden coffin in which they had been enclosed had molded away. The remains were placed in a metal casket and deposited in the mortuary chapel of the cemetery, where they rested until January 2, when the casket was enclosed in a coffin of strong wood and covered with the American flag by Consul Bishop. On this occasion Doctor Bell, Mr. Bishop, and the other witnesses again assembled, and the following remarks were made:

Remarks by William Henry Bishop, Esq., United States Consul.

DOCTOR ALEXANDER GRAHAM BELL: You arrived here, my dear Dr. Graham Bell, charged by the Smithsonian Institution with the mission of removing to Washington the remains of the Founder of that Institution, James Smithson, who has been buried till now in the cemetery where we stand, since his death, at Genoa, in the year 1829. Having been invited by you and by the Smithsonian Institution to aid you, to what extent I might be able, in this object, it has been a matter of great pride and pleasure to me that I have been allowed to do so.

All the steps necessary to such removal have now been taken. We have received the authorization of the governmental heads of the Province, the City, and the British Burial Ground Fund, in which latter the title to the cemetery and the custody of the grave of James Smithson are vested, and all of these have kindly coöperated with us in the work.

The body of James Smithson has now been reverently raised from the earth; it has been placed in a case securely sealed, and this case stands ready to pass into the charge of the Steamship Company which will convey it to New York.

I assure you that it is with a feeling of real emotion that I have just now cast the American flag over the body of this illustrious man, this noble but as yet little known benefactor, as it is on the verge of beginning its journey to the United States. The flag adopts him already, as it were, in the substance, for our country, to which he has so long belonged in the spirit. He is now about to receive there a portion of the outward veneration and homage he so supremely merits, and which, owing to the modest circumstances of his life, and his interment here in some sense almost forgotten, he has never had.

Shall I admit that on taking possession of my post as Consul at Genoa, I did not even know who James Smithson was? I may say that I was surprised to learn that he was buried at Genoa; more surprised still that he was an Englishman, who had never even set foot in America. He left his great bequest to the United States, then in its infancy, through admiring confidence in our future. It is likely that many, or even most, Americans are in the same condition as was I myself; for occasion has rarely arisen for taking thought as to the personality of the man. Happily this unenlightened condition of mind is about to cease.

Dr. Graham Bell, I wish you a hearty God-speed across the ocean, with your precious freight. The American people will receive it with general gratification, and, through the Smithsonian Institution, will soon delight to pay it great honor.

Response by Doctor Alexander Graham Bell.

MR. CONSUL: It is with feelings of deep emotion that I undertake the transportation of the remains of James Smithson from the cemetery where they have so long reposed, to their last resting place in the United States.

On behalf of the Smithsonian Institution allow me to thank you, Mr. Consul, for the unwearied zeal and care with which you have given me your assistance. Without your active coöperation—and without your personal sympathy—it would have been difficult indeed for me to have accomplished the object of my mission here.

On behalf of the Smithsonian Institution, I beg to thank you too —Mr. Noel Lees—for your courtesy and attention; and trust that you will convey to His British Majesty's Consul General, and to the Committee of the British Church Burial Ground Fund, my thanks, and the thanks of the Institution I represent, for their ready assistance in furthering my mission.

The United States of America will provide in Washington, D. C., a suitable and permanent resting place for the remains of her great benefactor, James Smithson, through the instrumentality of the Smithsonian Institution—the Establishment created by the Government to perpetuate his name.

Remarks by Noel Lees, Esq.

Doctor Graham Bell: I beg to thank you heartily for the words you have said with regard to the aid you have received from the Burial Board and myself. Although we regret to lose the remains of James Smithson, we at the same time feel that in the country to which he left his money, with such charitable intent, his remains will receive the honor and glory which have so long been due to them, and we must understand that our loss is America's gain. To us it will always remain a pleasant memory that, from the date of his burial to the present day, we have had in our custody in this picturesque little church-yard, the remains of a man whose foresight and kindness have enabled so many in the New World to benefit.

On the conclusion of these remarks the remains were placed on board the steamer *Princess Irene* of the North German Lloyd line, which brought them to New York, where they arrived on the night of January 19, in the continued charge of Doctor Bell, the vessel reaching her dock at Hoboken early on the morning of the 20th. By direction of the President of the United States, the U. S. steamer *Dolphin* had been detached to meet the *Princess Irene* in the lower bay and to accompany her up the harbor, while a tug belonging to the Navy Yard attended at the dock to receive the remains and transport them to the *Dolphin*. They were received by Mr. Bell and the Secretary of the Institution, Mr. Bell accompanying the remains to the *Dolphin* and taking passage on her himself for Washington, where she arrived at the Navy Yard on Saturday the 23d.

On Monday the 25th the remains were transported by the Naval authorities, with suitable ceremonies, to the Navy Yard gate, where they were taken in charge by a cavalry escort furnished by the War Department, and, accompanied by Assistant Secretary of State Loomis, representing the President, by the British Ambassador, the Regents and the Secretary of the Institution, and the

President of the Board of Commissioners of the District of Columbia, they were conveyed to the Smithsonian Institution, where the coffin, draped in the American and British flags, was deposited in the center of the Main Hall of the building.

Dr. Alexander Graham Bell, addressing Senator Frye in behalf of the Regents, said:

MR. SENATOR: I have the honor to hand over to the Smithsonian Institution the mortal remains of its founder, James Smithson, a Fellow of the Royal Society of London, England, who died in Genoa, Italy, on the 27th of June, 1829.

For nearly seventy-five years the body of Smithson has reposed in an almost forgotten grave in the picturesque little British cemetery on the heights of San Beningo in Genoa. City improvements have led to the expropriation of this cemetery and the removal of the remains, and at the last meeting of the Board of Regents of the Smithsonian Institution I was appointed a committee to arrange for the transfer of the remains of Smithson to this country. On my arrival in Genoa, every facility was afforded me for the accomplishment of my mission by the provincial and municipal authorities, by His British Majesty's Consul-General, Mr. Keene; by the Committee of the British Burial Fund Association, in which is vested the ownership of the cemetery, as well as by our own Consul, Mr. William Henry Bishop, to whom I am much indebted for his valued services.

On the 31st of December, 1903, the tomb of Smithson was opened in my presence, as the representative of the Smithsonian Institution, and in the presence of the American Consul and six other witnesses. The remains of Smithson were reverently raised from the grave and placed in a metallic casket, over which the Consul of the United States cast the American flag while the witnesses stood around with uncovered heads. The casket was then left in the mortuary chapel of the cemetery, securely sealed and under guard, until the 2d of January, when it was placed in a coffin of strong wood, as demanded by Italian law, and was then transported to the North German Lloyd steamship *Princess Irene,* accompanied by the American Consul and myself.

The steamer sailed from Genoa on the 7th of January, and upon arrival in the United States, the remains of Smithson were received with national honors by direction of the President, and of the Secretary of the Navy and the Secretary of War.

The remains were brought to Washington on board the United States dispatch boat *Dolphin,* and have been escorted to the Smithsonian Institution by United States cavalry.

And now, Mr. Senator, my mission is ended, and I deliver into your hands, as the representative of the Board of Regents of the Smithsonian Institution, the remains of this great benefactor of the United States.

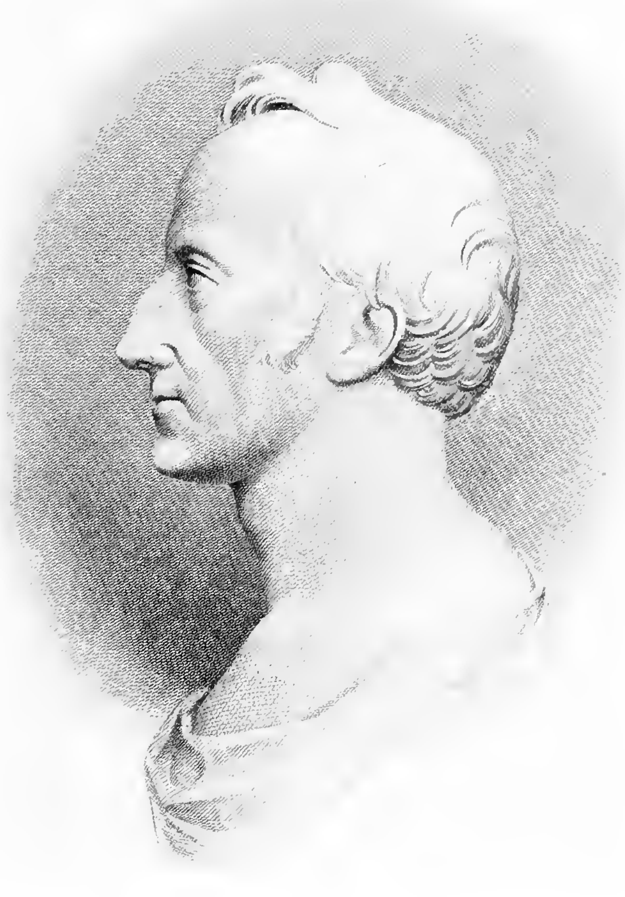
Senator Frye replied:

SIR, the Smithsonian Institution receives with profound gratitude the remains of its distinguished founder. Providence, every now and then, seems to place in the world a man and inspires him with a purpose to elevate his fellow men. Such a man was Mr. Smithson, the founder of this Institution. The spirit, Sir, which prompted you to such earnest endeavor, resulting as it did in taking these remains from their resting place in a country foreign to him and foreign to us, and bringing them here where for so many years we have enjoyed the rich fruits of his splendid benefaction, your countrymen will appreciate. His grave here will be an incentive to earnest, faithful, wise, and discreet endeavor to carry out his lofty purposes, and, Sir, it will be to our people a sacred spot while the Republic endures.

The brief but impressive ceremonies of the occasion concluded with the following prayer, offered by the Reverend Doctor Randolph H. McKim:

Almighty God, eternal source of light and truth, by whose wise providence all things in heaven and earth are governed, we give Thee thanks that Thou didst put into the heart of Thy servant whose dust we receive with reverence here to-day, to lay the foundation of this school of science, and we pray Thee that it may more and more be instrumental in the true interpretation of the laws of nature, and in unveiling to the mind of man the glory of God in the work of His hands, to the end that for all the generations to come, this Institution may be a beacon light of truth and of progress, to the glory of God and to the good of mankind. All this we beg through Him by whom all things were made, Jesus Christ, our Lord. Amen.

The remains rest temporarily in a room which contains the few personal relics of Smithson, until their final disposal by the Regents



JAMES SMITHSON

FOUNDER OF THE SMITHSONIAN INSTITUTION

JAMES SMITHSON

BY

SAMUEL PIERPONT LANGLEY

REPRINTED FROM "THE SMITHSONIAN INSTITUTION, 1846–1896
THE HISTORY OF ITS FIRST HALF CENTURY"
EDITED BY G. BROWN GOODE

(1442)

CITY OF WASHINGTON
1904

JAMES SMITHSON

By Samuel Pierpont Langley

THE founder of the Smithsonian Institution was known in his earlier years as James Lewis Macie, his mother, Elizabeth Keate Macie, being at the time of his birth, in 1765, the widow of James Macie, a country gentleman of an old family resident at Weston, near Bath. She was of the Hungerfords of Studley, a great-grandniece of Charles, Duke of Somerset, through whom she was lineally descended from Henry the Seventh, and was cousin of that Elizabeth Percy who married Hugh Smithson (who later became Duke of Northumberland, and by act of Parliament took the name of Percy).

An unverified story represents Smithson's mother as at one time hoping to have contracted a marriage with the Duke of Northumberland, and seeking, for that purpose, a divorce from her husband, which he successfully opposed; but, in any case, the subject of our sketch, who only apparently after his mother's death applied to the Crown for permission to take the name of Smithson, describes himself in his final will as "son to Hugh, first Duke of Northumberland, and Elizabeth, heiress of the Hungerfords of Studley, and niece of Charles, the proud Duke of Somerset."

We need not, then, practise a reticence which Smithson himself did not desire to observe, especially since the facts are already public. There is, indeed, the further reason that it is especially to these facts that the foundation which bears his name is due, for Smithson always seems to have regarded the circumstances of his birth as doing him a peculiar injustice, and it was apparently this sense that he had been deprived of honors properly his which made him look for other sources of fame than those which birth had denied him, and constituted the motive of the most important action of his life, the creation of the Smithsonian Institution.

By the student of human nature every man's conduct is judged in reference to its determining motives, and if we try Smithson's from the point of view of his own time, not of ours, we shall not judge too hardly the fact that the circumstances of his birth and his feeling that he was by right a Northumberland and a Percy were a subject of pride to him as well as of pain. He once wrote:[1]

> "The best blood of England flows in my veins; on my father's side I am a Northumberland, on my mother's I am related to kings,[2] but this avails me not. *My name shall live in the memory of man when the titles of the Northumberlands and the Percys are extinct and forgotten.*"

It has been wondered that Smithson should have left his fortune for the purpose he did, but not by those who have considered the sentence placed here in italics, where we surely scarcely need to read between the lines to see the genesis of the institution which perpetuates the name he bore, in place of the titled one he was denied.

It will be observed from facts given later that it was only under circumstances which showed that he had no right to the name of Macie (which seems to have been first imposed upon him under circumstances which left him free to change it) that he in later life had that of Smithson, to which he had every moral right, legally confirmed to him. After pointing out that the change was obtained under circumstances which do him no discredit, we are chiefly concerned with this sense of the injustice under which he labored from its after results; for if the kind of pride which dictated the first sentence I have above quoted be one which, from the point of view of the present day, attracts little sympathy, we can feel more with the worthier spirit which resulted from it, and in which he wrote the second. We are in no ways concerned with the ancestral honors or titles of the Percys, as such; but if there be anything in heredity, we may supplement our limited knowledge of him by some consideration of that very remarkable man, the first Duke of Northumberland, whose child Smithson declared himself to be, and undoubtedly was; for the father was remarkable, not in having been born great, but in having achieved greatness—at least a greatness of that sort which his less fortunate son must always have envied him.

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[1] Rhees's "Smithson and his Bequest." "Smithsonian Miscellaneous Collections," volume XXI.

[2] Doctor Goode pointed out in his "Account of the Smithsonian Institution," written for the Atlanta Exposition, that "Smithson was of royal descent, through his maternal ancestor, the ill-fated Lady Jane Grey, great-granddaughter of King Henry VII, grandniece of Henry VIII, and cousin of Elizabeth. His ancestor in the ninth generation, Edward Seymour, the first Duke of Somerset and Protector of England, was the brother of Queen Jane Seymour and the uncle of King Edward VI."

Hugh Smithson, the father of the founder of the Smithsonian Institution, was the son of Langdale Smithson, who, according to another unverified tradition, occupied for a time the then relatively unconsidered position of a medical practitioner. The Smithsons, however, were an old family, which was, in fact, remotely connected by lineage with the Percys. As country gentlemen they were reared in the habit of personally managing their estates; and, notwithstanding his culture and his refined and artistic tastes, the business aptitude of his race was strong in Smithson's father.

The entertaining story of his courtship of the grand-daughter of "the proud Duke" of Somerset is told in the "Annals of the House of Percy," and it is not necessary to repeat it here further than to remark that in it, as in everything else, he showed the tact, persistence, and ability which raised him from the position of a private gentleman to one of the first dukedoms of England at a time when such a transition was regarded as transcending all possibility, and became the subject of wonder after it had happened.

As a landlord, Sir Hugh Smithson (as he afterwards became)[1] had been conspicuous for good management. After his marriage to the heiress of the Percys he restored Alnwick Castle, and lived there so expensively that Horace Walpole wrote of the new groom and bride that they would soon have no estate left; but the prophecy was falsified by the marked ability of the future Duke, who, though he continued to maintain what was even then considered magnificent state, showed such extraordinary administrative capacity as enabled him not only to keep undiminished but to very greatly increase the important possessions which became his wife's after their marriage; for at the date of Sir Hugh Smithson's marriage, in 1749, the rent rolls of Alnwick Castle amounted to £8,607, while in 1778 they had increased to £50,000, and all this while a liberal and even magnificent scale of expenditures appears to have been adopted.[2]

If he be a benefactor to mankind who makes two blades of grass grow where one grew before, then the new Lord of Northumberland did indeed entitle himself to the gratitude of those within the influence of his kindly rule.

"He found the country almost a desert," says the Bishop of Dromore, "and he clothed it with woods and improved it with agriculture."[3] For more than twenty years he is said to have planted annually over twelve hundred trees; he imported specimens of hitherto unknown timber, fruits, and flowers from various parts of the world, and expended large sums not only in the reclamation and drainage of lands, but in the improvement of the dwellings of his laborers, at a time when the physical comfort or moral well-being of the poor rarely occupied the thoughts of the lords of the soil.

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[1] He succeeded to the title of Baronet on the death of his grandfather, Sir Hugh Smithson, which took place in 1729.

[2] See "Annals of the House of Percy," by Edward Barrington de Fonblanque, London, 1887, Volume II, page 531, and Appendix XXVI.

[3] See "Annals of the House of Percy," by Edward Barrington de Fonblanque, London, 1887, Volume II, page 531, and Appendix XXVI, citing Collins's [Peerage] 5th edition.

He showed a like ability in his dealings with the Crown, which procured him the unprecedented step from the baronetcy to the dukedom, and in every part of his life (with which we are not further concerned here) he showed himself an exceptionally able man.[1]

American history and poetry remember his son, the half-brother of Smithson, who—

> " Fought for King George at Lexington,
> A Major of Dragoons,"[2]

and who, it might be added, saved to the King the remnant of his forces, which, without Percy's timely succor, would have been utterly destroyed. As an indication of family traits, it may be interesting to note the memorable action of the half-brother of Smithson, and his modest description of it.

General Gage had placed him in command of the camp formed at Boston, whence he writes to his father on July 5, 1774:

"As I cannot say this is a business I very much admire, I hope it will not be my fate to be ordered up the country. Be that as it may, I am resolved cheerfully to do my duty as long as ever I continue in the service. If I do not acquire any degree of reputation in it, it will be my misfortune, but shall never be my fault."

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[1] The Duke showed the independence of his character, as well as the soundness of his judgment as a statesman, by opposing the party in power upon the question of war with the Colonies, obtaining leave of absence for his son, Lord Percy, who was ordered to America. Of this, however, Lord Percy refused to avail himself, contending that he could not at such a juncture withdraw. He accordingly embarked for Boston in the spring of 1774, and his journal and letters during the succeeding years throw light upon many of the incidents of the struggle.

[2] The fact that the heir of the house of Percy commanded the force of the British troops which saved the retreat from Concord made a strong impression upon the fathers of New England who fought on the memorable day, and is often mentioned. This association of the story of the defeat and pursuit of the British troops with the name of Percy, in the minds of the rustic victors, is alluded to by Lowell:

> "Old Joe is gone, who saw hot Percy goad
> His slow artillery up the Concord road. . . .
> Had Joe lived long enough, that scrambling fight
> Had squared more nearly with his sense of right,
> And vanquished Percy, to complete the tale,
> Had hammered stone for life in Concord jail."

Throughout the ensuing winter he remained in the camp around Boston, whence on April 20 he writes to inform his father of that first bloodshed which was the prelude of the War of the Revolution:

"I was ordered out yesterday morning to cover the retreat of the Grenadiers and Light Infantry who had been sent upon an expedition up the country.[1] I had with me my brigade and two pieces of cannon. We met them at a town[2] about fifteen miles off, sharply attacked and surrounded by the rebels, and having fired away all their ammunition, I had the happiness of saving them from inevitable destruction, and arriving with them at Charlestown, opposite Boston, at eight o'clock last night; not, however, without the loss of a great many, having been under an incessant fire for fifteen miles. The rebels, however, suffered much more than the King's troops. I have not myself received even the least scratch, and I beg that you will not either of you be uneasy on my account."[3]

Lord Percy was too good a soldier to fall into the error of despising his enemy. He had never shared in that contemptuous estimate which Englishmen, ignorant of the country and its population, had formed of the military capacity of the American colonists, and which had led the King, under the prompting of such advisers as Lord North and Lord George Germain, to declare that all resistance would collapse on the first menacing advance of half a dozen English regiments.

"Whoever," he writes to his father, "looks upon them as merely an irregular mob will find himself much mistaken; they have men amongst them who know what they are about, having been employed as rangers against the Canadians and Indians . . . nor are their men devoid of the spirit of enthusiasm, as we experienced yesterday, for many of them concealed themselves in houses and advanced within ten yards to fire at me and the other officers, though they were morally certain of being put to death themselves in an instant."

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[1] The memorable expedition to Concord, which gave rise to the battle of Lexington.

[2] Lexington.

[3] "Annals of the House of Percy," Volume II, page 552.

The father died in 1786, and was buried in Westminster Abbey, where he is described as "the most high puissant and most noble prince Hugh Percy, Duke and Earl of Northumberland, Earl Percy, Baron Warkworth and Lovaine, Lord Lieutenant and Custos Rotulorum of the Counties of Middlesex and Northumberland and of all America, one of the lords of His Majesty's most Honourable and Privy Council and Knight of the most noble Order of the Garter, etc., etc., etc."; but we are here concerned with these honors only as an evidence of the character of the man who did not inherit, but who conquered them by the force of his will.

Let us, after noting the essential qualities of his race in the father and brother, return to the immediate subject of our memoir, the date of whose birth is fixed by the Pembroke College record as 1765. His mother, Elizabeth Hungerford Keate (Macie), is described in the will of Penelope Keate, grandmother of Smithson, in a bequest dated July 13, 1764, as "my daughter, Elizabeth Macie, of Bath, widow," so that at this time her husband was already dead. This fact, only recently ascertained, is important in the estimate it leads us to put on one of the principal actions of Smithson's life, his taking of his father's name instead of that of Macie, by which he was previously known.

Something of the facts of the young man's birth were generally surmised, and we shall see that he was apparently not allowed as a youth even to describe himself as Macie's son, a thing to be remembered in connection with his subsequent action in taking the name of Smithson.[1]

There has been found no record of the Macies at Weston in the years preceding his birth; there is no reference to him in the accessible archives of the Northumberland family, nor do we know more of the subsequent circumstances of his mother than that she inherited the property of the Hungerfords of Studley in 1766, on the death of her brother, Lumley Hungerford Keate,—a matter of interest as indicating the probable source of a considerable portion of the Smithson bequest.

We have after this no knowledge of the founder of the Institution until his name is entered in 1782 as James Lewis Macie, a Gentleman Commoner, at Pembroke College, Oxford, but entered in a way which, as the copy of the record indicates, omitted the prescribed form of stating the name of the father, which others were obliged to comply with.

He was at this time but a lad, and as we are assured only very powerful influence could have procured permission for this departure from rule, we may presume that his action, whether acceptable or not to him, was dictated by an authority to which he had in any case to yield.

In 1894 I ascertained through the kindness of Chester Waters, Esquire, that Reverend Frederick Brown had occupied himself during a large part of his life with the biographies of the Hungerford family, and learned from his surviving daughter that his manuscript was deposited in the British Museum. This manuscript (which is numbered 33,412), I, with Doctor Cyrus Adler, spent some time in examining, with the results here given. Among other facts I learned that Smithson was born in France, and was brought to England for his education, and naturalized. I further was fortunately led to consult the Oxford records, which show that he in his early years entered as a Gentleman Commoner at Pembroke College, where he matriculated in 1782, his age then being given in the registry, here appended,[1] as seventeen, so that this matriculation record shows him to have been born eleven years *later* than was supposed. This is material, for it will be seen from what has preceded that his memory is thus cleared of the imputation under which it at one time seemed to rest, of his having adopted the name of Smithson in circumstances where a son should have remained silent.

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[1] In 1880, when Mr. Rhees's memoir was prepared, the date of Smithson's birth, obtained from an erroneous inscription on his tomb, was 1754, which would have placed it eleven years earlier than the actual event, during the married life of Mr. and Mrs. Macie, and put a less favorable construction on young Macie's action in taking the name of Smithson from that it bears, under the circumstances which are now for the first time detailed.

We have also an authentic contemporary portrait of him in the dress of an Oxford student, here reproduced [omitted in this reprint], which, it is interesting to observe, confirms the age thus given, by representing him as a mere youth.

Nothing material is remembered of his life at the college, except a tradition that he was the best chemist and mineralogist of his year, though in his journal, when but a youth of nineteen, he gives a description of a geological tour in 1784 through Oban, Staffa, and the western islands, in company with De St. Fond, "the celebrated French philosopher," and the Italian Count Andrioni, in which he carried on observations on the methods of mining and manufacturing processes, made with all the minuteness which the conditions of the journey permitted. The journal indicates that the tour at that time was undertaken, if not at any considerable risk, yet not without a considerable amount of privation and self-denial, such as would not be met by the modern traveler, and shows that he was far more occupied with science than with the ordinary pleasures of so youthful a tourist. We learn also that the young student was noted for diligence, application, and good scholarship, attracting attention by his proficiency in chemistry, then a novel study, while his vacations were ordinarily passed in such excursions as that just referred to, and devoted to the collection of minerals and ores, which it was his favorite occupation to analyze. At Oxford, then, at a time when the study of physical science was almost unknown in the University, he appears to have already conceived that devotion to scientific research which characterized all his future life.

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[1] Coll: Exon: 25^{o}. Carolus Ofspring Blackall 17 Theophili de Dodbrooke Com: Danmon: } Cler: Fil:

Coll: Wad: 26° Robertus Harbin 17 Swayne de Newton Com: Somerset: Arm: Fil: Maii 1^{o}.

Coll: Hert: Gulielmus Bragge 17 Joannis de Dillington Com: Somerset: Arm: Fil:

Coll: Wadh: 2do. Joannes Higgins 19 Joannis de Dicheatt Com: Somerset: —— Gen: Fil:

Coll: Mert: 3^{o}. Henricus Lloyd 18 Erasmi de Civitate Vigorniensi Gen: Fil:

Coll: Di. Jo. Bap. 4^{o}. Thomas Keck 17 Samuelis de Civitate Londin: —— Gen: Fil:

changed to Smithson

Coll: Pemb: 7° Jacobus Ludovicus Macie 17 de Civit: Londin: — Arm: Fil:

Coll: Ball: 8^{o}. Hon. Archibaldus Cathcart 18 Caroli de Aloa Com: Clackmanan: Baro^{s}. Fil

Coll: Di: Jo: Bap. 9^{o}. Thomas Dethick 17 Thomae de Bombay apud Ind: Orient: Arm: Fil

Coll: On: Nas: 10mo. Arthurus Townson 18 Joannis de Bentham Com: Eboracensi —— Pleb: Fil

Coll: Christi 10mo. Calverley Joannes Bewicke 17 Benjamin de Clapham Com: Surriae Gen: Fil:

Coll: Magd: 11^{o}. Isaacus Williamson 21 Josephi de Withburn Com: Cambr: Gen: Fil:

He was graduated at Pembroke College, with the degree of Master of Arts, on May 26, 1786, as James Lewis Macie, and admitted as a Fellow of the Royal Society on April 26, 1787, on the following recommendation:

"James Lewis Macie, Esq., M. A., late of Pembroke College, Oxford, and now of John Street, Golden Square,—a gentleman well versed in various branches of Natural Philosophy, and particularly in Chymistry and Mineralogy, being desirous of becoming a Fellow of the Royal Society, we whose names are hereunto subscribed, do, from our personal knowledge of his merit, judge him highly worthy of that honour and likely to become a very useful and valuable Member.

RICHARD KIRWAN,
C. F. GREVILLE,
C. BLAGDEN,
H. CAVENDISH,
DAVID PITCAIRN."

Cavendish, whose name appears here, was the eminent physicist, and, as we learn elsewhere, was an intimate friend.

Smithson's lodgings were for some time in Bentinck Street, where Gibbon wrote much of his "Decline and Fall of the Roman Empire." Here he apparently prepared his first scientific paper, which was signed James Lewis Macie, and was read on July 7, 1791, before the Royal Society. It is entitled "An Account of Some Chemical Experiments on Tabasheer."[1] We learn of him incidentally in 1792 as journeying from Geneva to Italy through the Tyrol, and find him in the same year in Paris writing from the Hôtel du Parc Royal, Rue de Colombier, a letter in which he expresses sentiments which represented what would have been then called advanced Jacobinism. "*Ça ira*," he says, "is growing the song of England, of Europe, as well as of France. Men of every rank are joining in the chorus. Stupidity and guilt have had a long reign, and it begins, indeed, to be time for justice and common-sense to have their turn . . . the office of king is not yet abolished, but they daily feel the inutility, or rather great inconvenience, of continuing it, and its duration will probably not be long. May other nations, at the time of their reforms, be wise enough to cast off, at first, the contemptible incumbrance." Smithson here shares the opinion of a large and influential portion of Englishmen of the time in which he wrote, but the excesses of the French Revolution, which immediately followed, caused a general revulsion of feeling, and it would not be fair to argue from this youthful expression as to his maturer judgment.

The date of his application to the Crown for permission to take his father's name has not been ascertained, but in the will of his half-sister, Dorothy Percy, he is referred to as "Macie" in 1794 (eight years after his father's death). The name of Smithson is first certainly known to have been used by him in connection with his second communication to the Royal Society, "A Chemical Analysis of Some Calamines,[1] by James Smithson, Esquire," read November 18, 1802.

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[1] *Philosophical Transactions of the Royal Society of London*, Volume LXXXI, part II, page 368.

In this paper the author remarks that "Chemistry is yet so new a science; what we know of it bears so small a proportion of what we are ignorant of; our knowledge in every department of it is so incomplete, consisting entirely of isolated points, thinly scattered, like lurid specks on a vast field of darkness, that no researches can be undertaken without producing some facts leading to consequences which extend beyond the boundaries of their immediate object."

The Abbé Haüy had advanced the opinion that calamines were all mere oxides or "calces" of zinc. Smithson's analysis completely overthrew this opinion, and established these minerals in the rank of true carbonates, while his remarks on the action of the ores of zinc before the blowpipe evince much discernment; and the paper, on the whole, is altogether a creditable one.[2]

At this period he seems to have ceased his contributions to the Royal Society, and later we find his name more frequently in the "Annals of Philosophy," a journal of high character, where there is a communication from him dated Paris, May 22, 1819, on "Plombe gomme," and about the same time a paper on a native sulphuret of lead and arsenic, with numerous other papers, among which is one in 1822, "On the Detection of Very Minute Quantities of Arsenic and Mercury," where he contributed a method which was generally used by chemists until quite modern tests superseded it. The papers[1] in all number twenty-seven, of which eight here cited were published in the "Philosophical Transactions of the Royal Society," between the years 1791 and 1807, one in the "Philosophical Magazine" in 1807, and eighteen in "Thomson's Annals of Philosophy," between 1819 and 1825, and these all give the idea of an assiduous and faithful experimenter, an impression enlarged by the last one of the series, bearing date of June, 1824, which contains some observations on the formation of the Kirkdale Cave, forcibly combating (with what was then originality) the theories of the time, which referred the bones there found to "The Deluge."

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[1] *Philosophical Transactions*, Volume XCIII, page 12.

[2] Smithson's subsequent communications to the *Philosophical Transactions* are six in number:

"An Account of a Discovery of Native Minium," submitted in a letter dated from Cassel, in Hesse, March 2, 1806. (Volume XCVI, part 1, page 267.)

"On the Composition of the Compound Sulphuret from Huel Boys, and an Account of its Crystals," 1808. (Volume XCVIII, page 55.)

"On the Composition of Zeolite," 1811. (Volume CI, page 171.)

"On a Substance from the Elm Tree, called Ulmin," 1813. (Volume CIII, page 64.)

"On a Saline Substance from Mount Vesuvius," 1813. (Volume CIII, page 256.)

"A few Facts relative to the Coloring Matter of Some Vegetables," 1817. (Volume CVIII, page 110.)

A paper by him "On Quadruple and Binary Compounds, particularly Sulphurets," was also published in the "Philosophical Magazine," 1807. (Volume XXIX, page 275.)

"The most notable feature of Smithson's writings from the standpoint of the modern analytical chemist," says Professor Clarke,[2] "is the success obtained with the most primitive and unsatisfactory appliances. In Smithson's day, chemical apparatus was undeveloped, and instruments were improvised from such materials as lay readiest to hand. With such instruments, and with crude reagents, Smithson obtained analytical results of the most creditable character, and enlarged our knowledge of many mineral species. In his time the native carbonate and silicate of zinc were confounded as one species under the name 'calamine'; but his researches distinguish between the two minerals, which are now known as Smithsonite and calamine respectively.

"To theory Smithson contributed little, if anything; but from a theoretical point of view the tone of his writings is singularly modern. His work was mostly done before Dalton had announced the atomic theory, and yet Smithson saw clearly that a law of definite proportions must exist, although he did not attempt to account for it. His ability as a reasoner is best shown in his paper upon the Kirkdale bone cave, which Penn had sought to interpret by reference to the Noachian deluge. A clearer and more complete demolition of Penn's views could hardly be written to-day. Smithson was gentle with his adversary, but none the less thorough for all his moderation. He is not to be classed among the leaders of scientific thought; but his ability, and the usefulness of his contributions to knowledge, cannot be doubted."

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[1] These papers were collected and edited by William J. Rhees, and are contained in Volume XXI of the "Smithsonian Miscellaneous Collections," under the title of "The Scientific Writings of James Smithson" (1879).

[2] Communication from Professor Frank W. Clarke, Chief Chemist, United States Geological Survey.

The President of the Royal Society, in a necrology for the year 1829, associated the name of Smithson with those of Wollaston, Young, and Davy, saying that "he was distinguished by the intimate friendship of Mr. Cavendish, and rivaled our most expert chemists in elegant analyses"; while at the annual meeting of the Royal Society held on November 30, 1830, the President, Davies Gilbert, after referring to other members recently deceased, said:

"The only remaining individual who has taken a direct and active part in our labours, by contributing to the 'Transactions,' is Mr. James Lewis Smithson, and of this gentleman I must be allowed to speak with affection. We were at Oxford together, of the same college, and our acquaintance continued to the time of his decease.

"Mr. Smithson, then called Macie, and an undergraduate, had the reputation of excelling all other resident members of the University in the knowledge of chemistry. He was early honored by an intimate acquaintance with Mr. Cavendish; he was admitted to the Royal Society, and soon after presented a paper on the very curious concretion frequently found in the hollow of bambû canes, named *Tabasheer*. This he found to consist almost entirely of silex, existing in a manner similar to what Davy long afterwards discovered in the epidermis of reeds and grasses.

"He was the friend of Dr. Wollaston, and at the same time his rival in the manipulation and analysis of small quantities. *'Αγαθή δ' ἔρις ἥδε βροτοῖσι.*

"For many years past Mr. Smithson has resided abroad, principally, I believe, on account of his health; but he carried with him the esteem and regard of various private friends, and of a still larger number of persons who appreciated and admired his acquirements."

His writings exhibit clearness of perception, terseness of language, and accuracy of expression. He was an intimate friend of Cavendish, and later of Arago; he was a correspondent of Black, of Banks, of Thomson, and of most of the names then renowned to science, and he himself contributed in those early days honorably to the enlargement of those "lurid specks in the vast field of darkness," of which he spoke, towards the coming light.

His industry was the more creditable to him in that he was at this time a man of large means, with every temptation to devote himself to amusement, and this industry will be seen to be still greater when it is remembered that these published papers are but a small portion of his writings; for 200 manuscripts were forwarded to the United States with his effects, and, besides these, thousands of detached notes and memoranda.

Unhappily, with the exception of one small volume, of all these nothing remains, the whole of the originals having been destroyed in the disastrous fire at the Institution in 1865, just one hundred years from the date of his birth. We know something of these manuscripts from the paper by Mr. Johnson, who had access to them before the formation of the Institution, and from it we learn that they are connected not only with science, but with history, the arts, language, rural pursuits, gardening, the construction of buildings, and kindred topics, "such as are likely to occupy the thoughts and to constitute the reading of a gentleman of extensive acquirements and liberal views derived from a long and intimate acquaintance with the world," while his cabinet, which was also destroyed by the fire, is described as consisting of a choice collection of minerals, comprising probably eight or ten thousand specimens, in exceedingly perfect condition, including examples of most or the meteorites which had fallen in Europe during several centuries, and forming what was at the time very much the richest and rarest collection in the United States.

If, then, we ask whether Smithson had such a competent scientific knowledge as would enable him to use with deliberate choice the language of his will, we may answer with confidence that his was the knowledge of a professional student, and not of the amateur, that he was one who could well discriminate between what was best for the increase and what was best for the diffusion of knowledge, and that the capacity for terse expression already referred to shows itself eminently in the concision and clearness with which he expresses this distinction, in that most brief and most important of clauses of his will presently to be cited.

We have a likeness of him taken in the form of a bust, executed probably in the years when he was most active in these scientific labors. The precise date of the bust, a reproduction of which (on steel by Charles Burt, made in 1879) is here given, is unknown.

Of Smithson's subsequent life we know but little. His later years appear to have been tried by bad health and painful infirmities. During these years he seems to have resided chiefly in Paris, where he lived at Number 121 Rue Montmartre, and where he was in the habit of entertaining his friends. One gathers from his letters, from the uniform consideration with which he speaks of others, from kind traits which he showed, and from the general tenor of what is not here particularly cited, the impression of an innately gentle nature, but also of a man who is gradually renouncing, not without bitterness, the youthful hope of fame, and, as health and hope diminish together, is finally living for the day rather than for any future.

To this period belongs an interesting citation from Arago's eulogy on Ampère:

"Some years since in Paris I made the acquaintance of a distinguished foreigner, of great wealth, but in wretched health, whose life, save a few hours given to repose, was regularly divided between the most interesting scientific researches and gaming. It was a source of great regret to me that this learned experimentalist should devote the half of so valuable a life to a course so little in harmony with an intellect whose wonderful powers called forth the admiration of the world around him. Unfortunately there occurred fluctuations of loss and gain, momentarily balancing each other, which led him to conclude that the advantages enjoyed by the bank were neither so assured nor considerable as to preclude his winning largely through a run of luck. The analytical formulas of probabilities offering a radical means, the only one perhaps of dissipating this illusion, I proposed, the number of the games and the stakes being given, to determine in advance, in my study, the amount, not merely of the loss of a day, nor that of a week, but of each quarter. The calculation was found so regularly to agree with the corresponding diminution of the bank-notes in the foreigner's pocketbook that a doubt could no longer be entertained."

I owe to Doctor B. A. Gould the interesting statement that Arago was not merely an acquaintance, but an intimate friend of Smithson, and that Arago personally told him that "the distinguished foreigner" in question was Smithson himself, and added that Smithson resolved, not to absolutely discontinue play (in which he found the only stimulus which could make him forget his physical suffering), but to do so with a care that the expenditure for this purpose was a definite one, and within his means.

We see him next entering the confines of old age, approaching the task (with such enfeebled health, a solemn one) of making his last will, and looking back upon a life which his circumstances have made lonely, which has been uncheered by domestic affection, and which, though filled with honorable activities, has not brought the fame to which he once aspired with the hope that it would bring some compensation for the accident of birth.

The most important act of his life was the execution of this will, a copy of which follows:

THE WILL OF JAMES SMITHSON.

"I James Smithson Son to Hugh, first Duke of Northumberland, & Elizabeth, Heiress of the Hungerfords of Studley, & Niece to Charles the proud Duke of Somerset, now residing in Bentinck Street, Cavendish Square, do this twenty-third day of October, one thousand eight hundred and twenty-six, make this my last Will and Testament:

"I bequeath the whole of my property of every nature & kind soever to my bankers, Messrs. Drummonds of Charing Cross, in trust, to be disposed of in the following manner, and I desire of my said Executors to put my property under the management of the Court of Chancery.

"To John Fitall, formerly my Servant, but now employed in the London Docks, and residing at No. 27, Jubilee Place, North Mile end, old town, in consideration of his attachment & fidelity to me, & the long & great care he has taken of my effects, & my having done but very little for him, I give and bequeath the Annuity or annual sum of One hundred pounds sterling for his life, to be paid to him quarterly, free of legacy duty & all other deductions, the first payment to be made to him at the expiration of three months after my death. I have at divers times lent sums of money to Henry Honore Sailly, formerly my Servant, but now keeping the Hungerford Hotel, in the rue Caumartin at Paris, & for which sums of money I have undated bills or bonds signed by him. Now, I will & direct that if he desires it, these sums of money be let remain in his hands at an Interest of five per cent. for five years after the date of the present Will.

"To Henry James Hungerford, my Nephew, heretofore called Henry James Dickinson, son to my late brother, Lieutenant-Colonel Henry Louis Dickinson, now residing with Mr. Auboin, at Bourg la Reine, near Paris, I give and bequeath for his life the whole of the income arising from my property of every nature & kind whatever, after the payment of the above Annuity, & after the death of John Fitall, that Annuity likewise, the payments to be made to him at the time of the interest or dividends becomes due on the Stocks or other property from which the income arises.

"Should the said Henry James Hungerford have a child or children, legitimate or illegitimate, I leave to such child or children, his or their heirs, executors, & assigns, after the death of his, or her, or their Father, the whole of my property of every kind absolutely & forever, to be divided between them, if there is more than one, in the manner their father shall judge proper, or, in case of his omitting to decide this, as the Lord Chancellor shall judge proper.

"Should my said Nephew, Henry James Hungerford, marry, I empower him to make a jointure.

"In the case of the death of my said Nephew without leaving a child or children, or the death of the child or children he may have had under the age of twenty-one years or intestate, I then bequeath the whole of my property, subject to the Annuity of One hundred pounds to John Fitall, & for the security & payment of which I mean Stock to remain in this Country, to the United States of America, to found at Washington, under the name of the Smithsonian Institution, an Establishment for the increase & diffusion of knowledge among men.

"I think it proper here to state, that all the money which will be standing in the French five per cents. at my death in the names of the father of my above mentioned Nephew, Henry James Hungerford, & all that in my names, is the property of my said Nephew, being what he inherited from his father, or what I have laid up for him from the savings upon his income.

James Smithson. [L. s.]"

We see that he begins by recalling the parentage which had denied him the name of his father and the position in the world he believed should have been his, and, in the void places of father, brother, or family, he seems to look for some object of affection, and to find only an old servant (whom he remembers with thoughtful liberality) and a nephew, to whom he bequeaths his property. He has provided for the continuance of the property to any possible heir to this nephew, and there seems to remain nothing more.

But there must have remained, in the retrospect of such a life as his, a sense of failure of that purpose with which he entered it, when he hoped, with youthful ambition, to create a greater name than that which birth had denied him, and when he wrote, "My name shall live in the memory of man when the titles of the Northumberlands and the Percys are extinct and forgotten," and there must have come up on such an occasion the question whether this was, indeed, the end of hope and the time only for renunciation.

We see that he has not utterly renounced this hope even now; but it is so faint that he writes between a clause which concerns a legacy to a servant and one which concerns an investment in the funds, and, as it were, almost casually, the words which have perpetuated his name.

Probably no man ever made a more remunerative investment in the direction in which he would like best to see a return than was brought out by these words of Smithson, for we now all know that his bequest, when accepted by the United States Government, formed the initial step in the creation of an institution whose position has been altogether exceptional, for it is likely to remain without successor, as without precedent, in perpetuating, as it does, the fame of a private individual, whose wishes have been adopted and carried into effect by a great nation, which has consented to take the position of a guardian to a ward in the care of his property, and which has subsequently made his private fortune the nucleus to which have been added appropriations for objects of national importance, yet appropriations which are still administered in association with his name.

The will was proved in the Prerogative Court of Canterbury, the value of the effects being sworn to be under £120,000. The property disposed of by it is believed to have been received chiefly from Colonel Henry Louis Dickinson, a son of his mother by a former marriage, though he is known to have received a legacy of £3,000 from Dorothy Percy, his half-sister on his father's side; but, unless through this, it is proper to state that there is no indication that any portion whatever of the Smithson bequest was derived from the Northumberland family.

The motives which actuated Smithson in mentioning the United States as his residuary legatee, rather than any other government or institution, must remain in doubt, for he is not known to have had any correspondent in America, nor are there in any of his papers any reference to it or its distinguished men. In selecting the nation itself as the depository of his trust, he yet certainly testified his confidence in its institutions and his faith in their perpetuity, while it has not escaped attention that he uses language in the determining clause of his will remarkably similar to that already employed by Washington, who in his farewell address, says: "Promote, as an object of primary importance, institutions for the general diffusion of knowledge."

Smithson died June 27, 1829, at Genoa, Italy. He is buried in the little English cemetery on the heights of San Benigno, in a tomb which originally bore no reference to him as the founder of this Institution; but the Institution has recently placed a tablet there remedying this omission, has surrounded the tomb with evidence of continued care, and has placed in still further remembrance a similar tablet in the English church of the city.

Smithson's wishes have been carried out by those immediately administering them with a constant scrupulous thought of the intent of the founder, while in doing this the best results have flowed from a rigid construction of his own words, so briefly expressed, and from a division of the activities of the Institution into two great distinct but parallel paths, the "increase" and "diffusion" of knowledge.

What has been done in these two paths the reader may partly gather from this volume—in the former, from the various articles by contemporary men of science, describing its activities in research and original contributions to the increase of human knowledge; in the latter, in numerous ways,—among others, from the description of the work of one of its bureaux, that of the International Exchanges, where it may be more immediately seen how universal is the scope of the action of the Institution, which, in accordance with its motto, PER ORBEM, is not limited to the country of its adoption, but belongs to the world, there being outside of the United States, at the time I write, more than 12,000 correspondents, scattered through every portion of the globe; indeed, there is hardly a language or a people where the results of Smithson's benefaction are not known and associated with his name.

If we were permitted to think of him as conscious of what has been, is being, and is still to be done, in pursuance of his wish, we might believe that he would feel that his hope, at a time when life must have seemed so hopeless, was finding full fruition; for events are justifying what may have seemed at the time but a rhetorical expression, in the language of a former President of the United States, who has said, "Renowned as is the name of Percy in the historical annals of England, . . . let the trust of James Smithson to the United States of America be faithfully executed, . . . let the result accomplish its object, the increase and diffusion of knowledge among men, and a wreath more unfading shall entwine itself, in the lapse of future ages, around the name of Smithson than the united hands of history and poetry have braided around the name of Percy through the long ages past."

The principal sources of information for this chapter have been as follows:

1. *Gentlemen's Magazine* for March, 1830, page 275.

2. The documentary evidence which, though meager, may be found in the report of Richard Rush to the Department of State, in 1838.

3. The manuscripts and diary of Smithson, which are described as comprising about two hundred titles, besides numberless notes of an encyclopædical character, "such as are likely to occupy the thoughts of a gentleman of extensive acquirements and liberal views." These manuscripts were destroyed by the fire of 1865, but not until extended extracts had been made from them by Walter R. Johnson, a member of the National Institute of Washington, in whose possession the papers and books of Smithson remained until the formation of the Institution. The paper by Johnson will be found in Volume XXI of the "Smithsonian Miscellaneous Collections," and these lost papers are the original sources of some statements made here which can no longer be verified by comparison with the originals.

4. These sources are not only contained in, but are largely supplemented by, the excellent memoir on "James Smithson and his Bequest," by Mr. William J. Rhees, forming part of Volume XXI of the "Smithsonian Miscellaneous Collections," without which the biography of Smithson can hardly be written, and from which the writer has here frequently quoted textually, without other acknowledgment than this general and explicit one.

5. Another source of information is the researches made by the writer with the aid of Doctor Cyrus Adler, Librarian of the Smithsonian Institution, in England, in 1894.

THE EXHIBIT

OF THE

SMITHSONIAN ASTROPHYSICAL OBSERVATORY

CITY OF WASHINGTON
FOR DISTRIBUTION AT THE LOUISIANA PURCHASE EXPOSITION
1904

THE EXHIBIT OF THE SMITHSONIAN ASTROPHYSICAL OBSERVATORY

BY S. P. LANGLEY

* * *

OBSERVATORIES are principally of two kinds: first, those which pursue chiefly the old astronomy, like that at Greenwich, and which are for the most part engaged in determining the places of the sun, moon, and stars, and, secondly, the astrophysical observatories, of which there are fewer and which are not much older than the present generation. These latter are engaged in studying the heavenly bodies, not so much to know where they are, as what they are; to see, for instance, not where the sun is in the sky, but what it is made of, and what influence it has on our own terrestrial affairs. To this second class belongs the Smithsonian Observatory, some of whose work is exhibited here.

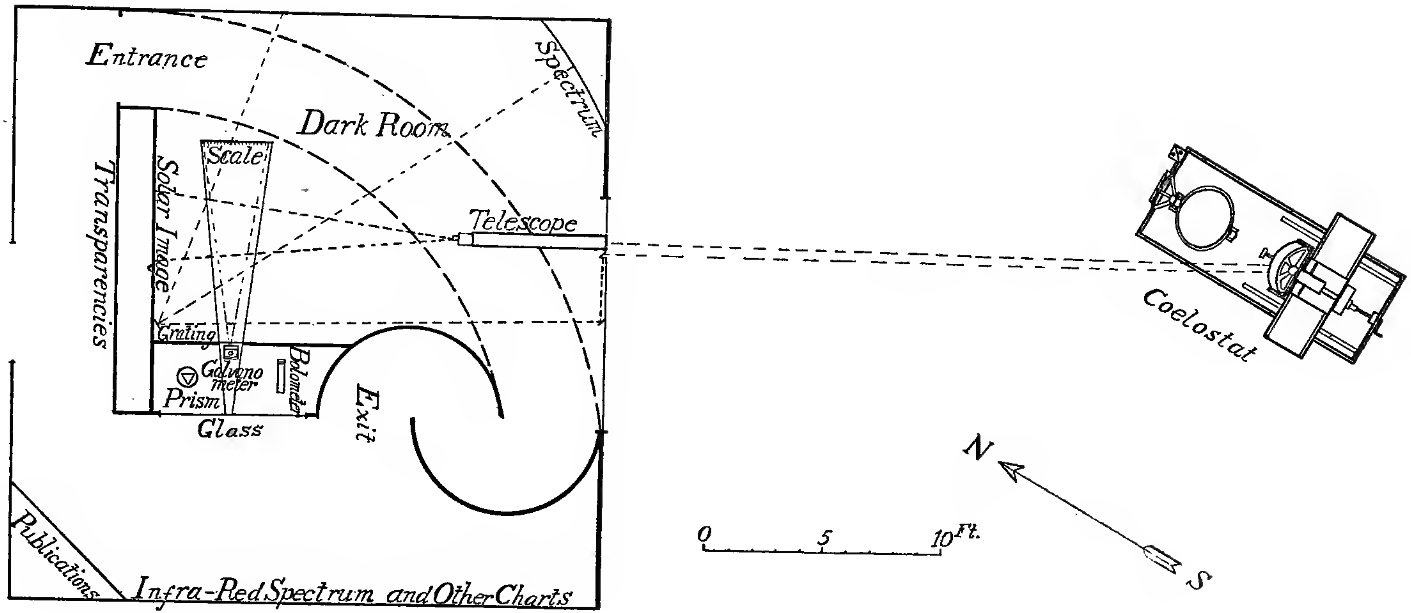
The Observatory Exhibit shown in the acompanying plan is divided into two portions, one of which is lighted, the other forming a "dark-room" for better showing the spectrum. Outside in the sunshine is an instrument which is called the coelostat, and which by means of mirrors throws a constant beam of sun-light (when there is any) into the dark-room. In this room it causes an image of the sun itself to be thrown upon a screen, and at the same time causes the solar spectrum to be thrown upon another screen on another wall.

The room should, if possible, be visited while the sun is shining brightly, for it is only then that its most attractive features can be seen.

THE SUN'S DISK.

Before the visitor, in the dark-room, will be seen, when the sun is shining, a bright circle of light, which is the sun's own picture cast by itself through a lens upon the screen.

This is not the place for facts to be better found in an Astronomy, but it may be well to remind the reader of what the actual enormity of the globe which he sees before him is, for it is so much larger than that of the moon, for instance, that if it were hollowed out and our own earth placed in the center, the moon might go on revolving in an orbit round the earth as great as its present one in the sky, all within the sun itself, without approaching to the outside. When we remember that everything that has life on the earth derives it from the sun, we understand better of how much practical use its study is to us, and why governments maintain observatories like this Astro-



Ground Plan of the Exhibit of the Smithsonian Astrophysical Observatory.

physical Observatory in Washington, which are chiefly engaged in its study.

On looking at the solar image attentively several interesting things may be seen, if the day be clear. One of these is the fact that the image is not quite equally bright all over, but is slightly darker toward the edges. This is because the sun has a gaseous envelope which at the edges acts like our own atmosphere, at sunset or sunrise, when it intercepts more light than at noon. This envelope is of the greatest consequence to us, and tempers to us the heat of the sun, which without it might make the earth unfit for habitation. There is some reason to suppose that this envelope varies in transparency, and that the radiation of the sun's heat through it, and the consequent temperature of the earth varies correspondingly, but this, though possible, is not certain.

At all times on the sun there may be seen on attentive examination, little irregularities all over the surface of the disk, caused by bright spots called faculæ, best seen near the edges, something in the sun brighter than the sun itself, and which are connected with the glow of metals existing there in a gaseous state.

Sometimes there may be seen spots, darker than the rest of the surface, and which slowly cross the disk, owing to the sun's rotation, taking about thirteen days to go over, while at the same time they move among themselves like clouds. They are supposed by many to be holes or depressions in the solar surface. Some of these are of enormous size; so large that the whole earth might be dropped into the center of one of them, like a pea into a thimble, the temperature that exists being so great that the earth would soon vanish in vapor if it were there. These spots are filled with curious and even beautiful details. Some drawings of them by the writer may be seen among the transparencies on the wall.

THE SOLAR SPECTRUM.

The bright rainbow-like colors made by the sun shining through a glass prism had been observed before Sir Isaac Newton's time, but he was the first to give an instructive account of them. This train of bright colors, beginning in the violet and ending in the deep red, is called the Solar Spectrum. It was found by Sir William Herschel to be continued by something which produces not color, but heat, for if a very delicate thermometer be moved along the spectrum formed by a prism, its temperature will be raised a little in the green, more in the yellow, and still more in the red, but be affected most of all when out of the visible spectrum altogether, quite below the red, where the eye sees nothing.

In the year 1814, a German named Fraunhofer, by using very perfect optical means, found that the visible spectrum of the sun and even of the stars, was everywhere crossed by black lines, definite as if made with ink by a drawing pen. These were incomprehensible until later, when it was shown that they were due to light from glowing metals in the sun and the stars, each metal having its own particular line or lines. This was the foundation of spectroscopy, which has revealed to us the constitution of these almost infinitely distant worlds.

The spectrum was produced by the prism only, until the time of Fraunhofer, who showed that it could also be formed by lines scratched on a glass or even on a metal surface; and in some respects the most perfect spectra are formed now in the latter way by instruments which are better made in this country than anywhere else. One of these is exhibited here, and it will actually form the visible and invisible spectrum when the sun is shining upon it. It is a great polished surface of what is called speculum metal, that is, a composition of copper and tin. On it are ruled with a diamond, parallel lines so fine that they are separately invisible to the eye, there being fourteen thousand of them in every inch. Here we see, if the sun favors us, its light broken up into those beautiful colors which exist in the pure light itself, every color in nature being derived from them. When the light is good we may follow the spectrum through this whole splendid display of color from the scarcely visible violet, successively through blue, green, yellow, orange, and red, to where it appears to end. We shall on looking closely at the spectrum actually before us, see a few of the lines referred to, which are called, from their discoverer, the Fraunhofer lines. Two of these are in the orange and are made by sodium in the sun. One of them, in the crimson red, is made by hydrogen. This metal grating, as it is called, is capable of showing thousands of them, but for that purpose the adjustment of the the apparatus would not be suitable to a public exhibition.

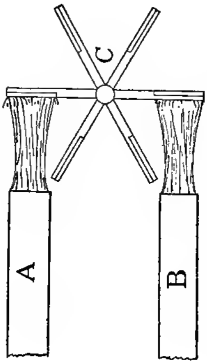
Now if the spectator can imagine a delicate thermometer passed along this spectrum from the blue to the red, he would see it rise higher and higher, and it would still show heat when it got beyond the red altogether. The spectrum, then, does not end with what Sir Isaac Newton saw, but there is something invisible beyond, and really there, although the eye cannot see it. If the thermometer be very delicate indeed, it will be found that in this invisible portion the heat rises, and then after falling, rises again, so that there are irregular fluctuations in it, but the best thermometer is not delicate enough to show them all.

THE BOLOMETER.

Some twenty-five years ago the writer, in trying to get a kind of heat measuring instrument adapted to these particular explorations, made one which he called the bolometer. A specimen of the bolometer is shown within the case on the right as the visitor leaves the darkened chamber.

The bolometer is an electrical instrument and its use is founded on the fact that when a current of electricity is flowing through a wire this flow is checked if the wire is heated, and diminished nearly in proportion to the heat which falls upon it. It is used in connection with the galvanometer, seen here near it, and which itself includes means of pointing out the amount and direction of the electrical current. This was formerly done by causing it to move a needle like that in the compass, but the needle is heavy and unsuited to indicate the almost infinitely delicate changes that come here into play. It is better to make a pointer out of something that weighs nothing at all, and this can best be done by taking a beam of light reflected from a mirror which casts it upon the screen where, as the mirror turns, the beam of light, which may be compared to a perfectly straight and weightless needle, points to some division of the scale. The mirror itself is not larger than the head of a pin. It is connected with some magnets hardly bigger than a needle's point, which are affected by the passage of an electric current near them, and cause the mirror to move and to deflect a beam of light upon the scale.

The way that this is done may be understood from an illustration. Let A and B be pipes through which two parallel equal currents of water are flowing, and let C be a wheel, like a horizontal mill-wheel, against which the two currents play; if these currents are just equal and exert their force on opposite sides, they do not move it, but if the current in one of the pipes, B for instance, be diminished, that in A will get the upper hand and turn the wheel; in this case in the direction of the hands of a watch. Now if we substitute for water the electric current and for pipes the wires (one of which wires may be heated and so diminish the flow of electricity through it), and for the mill-wheel we substitute a little mirror with its magnets, we see how the instrument may work.



In the very interior of the cylinder before the reader, and quite invisible, are two wires, or rather tapes, made out of extremely thin strips of platinum, two tiny ribbons in fact, in width like that of a human hair, but far thinner than wide. Only one of them receives the heat or cold, which may be ever so small in quantity, like that derived from the cold of one of the lines in the spectrum. Small as it may be, even less than a millionth of a degree, this instrument will indicate it. Its sensitiveness cannot be well shown to the public, but in order to give some notion of it, the visitor, looking through the plate glass, in which there is a small aperture, toward which the bolometer is directed, will see the spot of light upon the screen move when his hand is placed before the aperture, by the feeble heat which is sent from it to the bolometer. In this case the hand is warmer than the bolometer and the little spot of light moves accordingly; if it had been colder, it would move in the other direction.

The bolometer is, then, sensitive to heat as the eye is sensitive to light; what is light to the eye is warmth to it, and what is darkness to the eye is cold to it; so that when it is moved along the spectrum and comes to a black line, the instrument feels something cold, its index swings to the side of cold, and if this motion is registered, we have evidence that a line is there, though we cannot see it. This is not of so much use to us in the visible spectrum, where the eye can very well afford to do without it, but as soon as we get into the invisible portion beyond the red, it is invaluable, for the bolometer here becomes an eye that sees in the dark, and this part of the spectrum is found to be filled with lines, though invisible, the bolometer hunting them out by their temperature, and in this way it is found that we see only a small part of the spectrum as it actually exists.

Researches with the bolometer have been conducted for many years at the Smithsonian Observatory in this heat spectrum, which contains the greater part of the energy that the sun sends to the earth—more than all there is visible—and they have extended our knowledge of the spectrum to eight times the length that was known to Sir Isaac Newton.

CHART.

If the visitor will turn to the chart which is hung upon the wall, he will see a map upon an extended scale, nearly 24 feet long, of the discoveries made in the lower heat spectrum chiefly by the bolometer.

In order to understand the way in which it works, we may take as an illustration the charts which are sometimes given in financial columns of the newspapers, of the variations of the stock market, where a curve is drawn representing the stock, the curve going up high when the stock is high, and down when the stock is low. (It is just to say that the stock market has borrowed this from the astronomers and not the astronomers from the stock market.) But in any case the curves shown here indicate the amount of heat which the bolometer recognizes, going up when that increases, and down when it diminishes, and inasmuch as it is constantly increasing or diminishing with the presence of the lines we have spoken of, this curve is extremely irregular, as is seen, each irregularity corresponding to the existence of one of the lines which are indicated below. The colored portion below this curve indicates the part which we can see, and which the visitor has already seen, if the sun is shining.

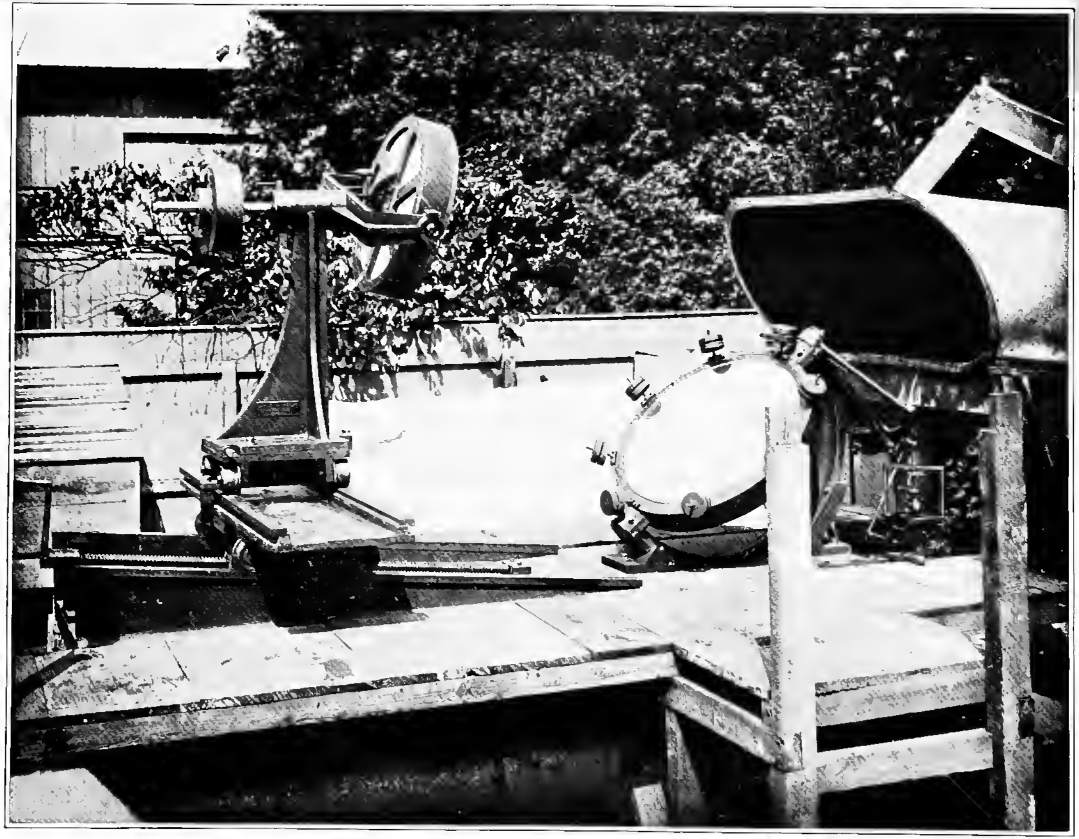
Part of these lines are caused by the metals in the sun, part of them are caused by the atmosphere of the earth, and to the latter is especially due the great depressions or valleys seen in the curve, all of which mean something of interest to us, for they are partly due to the moisture and other ingredients of our own atmosphere, and as they increase and differ from one day (and from one year) to another, they indicate changes in the conditions of our weather which it is not unreasonable to hope may enable us, when the studies are completed, to get a notion of coming changes, somewhat in the same way but more extensively than we now get a notion of from the Weather Bureau.

On entering the room the reader will notice a large group of transparencies representing the Observatory which is a very modest building without a dome for a great telescope, or many of the appurtenances of the ordinary observatory. Some of its principal instruments are also shown, including the great siderostat, the spectro-bolometer and a group of large cameras used in Sumatra to photograph the solar eclipse of 1901.

The siderostat, by Sir Howard Grubb of Dublin, Ireland, is an instrument invented by Foucault to furnish a horizontal fixed beam of light from any celestial object, but differs from the great new coelostat exhibited outside the Exposition building, in having but one mirror instead of two. This optical simplicity is counterbalanced by mechanical complexity so troublesome that it is not possible to obtain as great accuracy of guiding the beam with the siderostat as with the coelostat. A further disadvantage of the siderostat is that only one point in the field of view is fixed, and the others rotate about it with variable velocity, so that in a telescope pointed at the siderostat mirror at night, one star alone would appear fixed in position, but the others would describe circles about it. The coelostat on the contrary reflects a field of view in which all parts are fixed.

In the center of the group of transparencies is a large view of the solar corona as photographed at Wadesboro, North Carolina, in May, 1900. It should be remembered that this photograph is not enlarged but is the exact size of the original negative taken with a lens of 135 feet focus. The corona is one of the most interesting features of the sun, but is only seen during the somewhat rare moments of eclipses, when the moon in her orbit comes directly between the sun and earth and hides the glare of the disk of the sun itself. It is not known just what the corona is, but some think there are many particles of dust around the sun, which being intensely heated owing to their nearness to it, emit light of their own, while also reflecting some. Other observers, struck by the similarity of the polar coronal streamers to the Aurora Borealis seen at night in northern parts of the world, attribute the corona, like the northern lights, to electrical discharges in rare gases.

Other transparencies show the outer corona and also the great solar prominences, which are flames of metallic vapors occasionally shooting out from the sun to enormous heights. Some of those photographed during the solar eclipse of 1900 were fully 50,000 miles high, but prominences may be observed at any



The Large Cœlostat with Second Mirror, Smithsonian Astrophysical Observatory.

time by the aid of a special device called the spectroheliograph, and have been seen projected as high as 300,000 miles from the disk of the sun. The solar corona is a still more extensive solar appendage than the prominences and was photographed in 1900 to distances of nearly 3,000,000 miles from the sun.

On the right of the great photograph of the corona are transparencies showing results of bolometric and other observations, less striking as pictures, but no less interesting in what they represent. First we have a pair of bolographs on a much less extended scale than the great wall map already described, but covering the visible spectrum as well as the upper part of that beyond the red. The visible portion is disproportionately extended by the extreme dispersion of the prism in this region. The principal well-known dark lines, such as H and K due to calcium, D of sodium, C of hydrogen, and A of oxygen are here shown as depressions caused by coolness of these regions relatively to the rest of the spectrum.

Further on in the infra-red are seen the great depressions caused by the absorption of water vapor. One of these bands is illustrated in another transparency on a much larger scale, showing that far from being a single great band it is really composed of many small lines. To illustrate the extreme changes in these bands of absorption caused by the variations in the amount of water vapor in our atmosphere, a pair of bolographs taken on two equally clear days, one moist, the other dry, are contrasted. It has been found that variations of the amount of moisture contained in our air often cause changes of 10 per cent in the heat of the direct beam of the sun, although no apparent clouds are visible. If our eyes could see only by the kind of rays in these water vapor absorption bands, we should find many of what we now call our brightest summer days to be darker than moonlit nights. It is a curious fact that though these infra-red regions contain even more extent, as reckoned by wave-lengths, than does the familiar visible spectrum, and might, it would seem, have answered the purposes of vision, yet our eyes are adapted to perceive most readily precisely those regions, namely, the yellow and orange, in which the most considerable amounts of the sun's light remain after it has suffered the absorption of the atmosphere, and reached the surface of the earth.

A series of nearly symmetrical bolometric curves are shown in another transparency, from which it may be inferred how powerfully the sun's own envelope absorbs the solar rays. The heights of the curves represent the intensity of the light from different parts of the sun's image as we pass from one edge to another along the sun's diameter and thus observe the light through different thicknesses of the solar envelope. If the sun were as bright at the edge as in the centre, all the curves would become straight lines; but the edge is in reality so much duller than the centre that in some parts of the spectrum the curves fall off as much as 50 per cent quite close to the edge of the sun. The last curve shows how marked an effect of absorption is observed in crossing over a sun spot.

Sunspots are found to be much darker compared to the sun's surface around them if viewed by violet light than if observed by some infra-red ray. Transparencies prepared from the drawings of sunspots by the writer, already referred to, are shown.

One small transparency alone remains to be noticed. This exhibits a series of photographs of artificial stars by means of a telescope of forty feet focus. It is well known to telescopic observers that the images of stars are continually in a greater or less degree of fluttering and distortion caused by the inequalities which exist in the atmosphere, and which may be seen in an exaggerated way by viewing objects over a hot stove. Many years ago the writer saw the late Professor Draper employ a liquid prism in which he stirred the liquid to do away with similar inequalities of density which interfered with the best seeing. It occurred to the writer to try the same device with the air in telescopes, and the photographs show how much the definition was improved by this proceeding. Thus we reach the paradoxical result that still air may be attended with most unquiet seeing, while vigorously churned air yields sharp and steady images. If then it were possible to extend a tube like a great dew-cap from the telescope to the outer regions of our atmosphere, and to vigorously churn the air in this tube and within the telescope, good seeing would always be secured in clear weather. Fortunately the chief disturbances in the air are usually near the surface of the earth, so that even so short a tube as 50 feet is of much advantage. For a great horizontal telescope, like that of the Astrophysical Observatory which has a focal length of 140 feet, the churning of the air within the tube is often indispensable to good definition on the solar image.

The drawing at the left of the entrance shows how the tube of the horizontal telescope is constructed and how the devices for churning the air within the tube are applied. A powerful fan blower withdraws air from many points along the tube, and at the same time forces in air at intermediate points. Thus a vigorous circulation is constantly kept up during the observations.

Other charts upon the wall illustrate the manner of determining exactly how much heat the sun sends the earth, and what portion of it is absorbed within the atmosphere and does not reach the earth's surface in the direct beam. There are two distinct things involved in the determinations and both present much difficulty. First by means of an instrument called the actinometer, or better the pyrheliometer (literally "solar heat measurer") it is found at what rate the heat is received at the earth's surface. This is usually expressed in terms of the rise of temperature in degrees Centigrade which would be caused in a gram of water by absorbing in the water all the sun's heat which falls perpendicularly on a square centimeter of area in a minute of time. At stations near sea level the rise of temperature is about 1° Centigrade or 1°.8 Fahrenheit, but as the observer ascends a high mountain the observed amount grows larger and larger, because there is less and less air interposed between him and the sun to cut off the rays by absorption. But all the solar rays, whether absorbed at the earth's surface or in the air above it, are of importance to climate and more or less directly to vegetation; hence it becomes a second and most valuable object of investigation to determine how much is lost in the air. This amount is found to vary greatly at different times, not only from clouds and from invisible moisture in the air, but from dust and smoke. Generally, however, in passing to sea-level the sun's beam loses nearly or quite half its heat in the air.

If we could ascend to the outer limit of the air the exact amount of atmospheric absorption would at once be determined. But this is impossible, and even observations on high mountains are difficult, consequently we must attempt to determine the amount lost in the air by trying the effect of different thicknesses of it. Such measures can be made on the rays of the sun at different hours of the day, for as it approaches the horizon the sun shines through thicker and thicker layers of air. But though a formula has been found which ought in theory to represent the results of such observation, yet even in theory it is applicable only to a light of a single color, not to the whole spectrum indiscriminately. Accordingly we must make our measures on the intensity of all parts of the solar spectrum at different hours of the day. This we do by means of the bolometer.

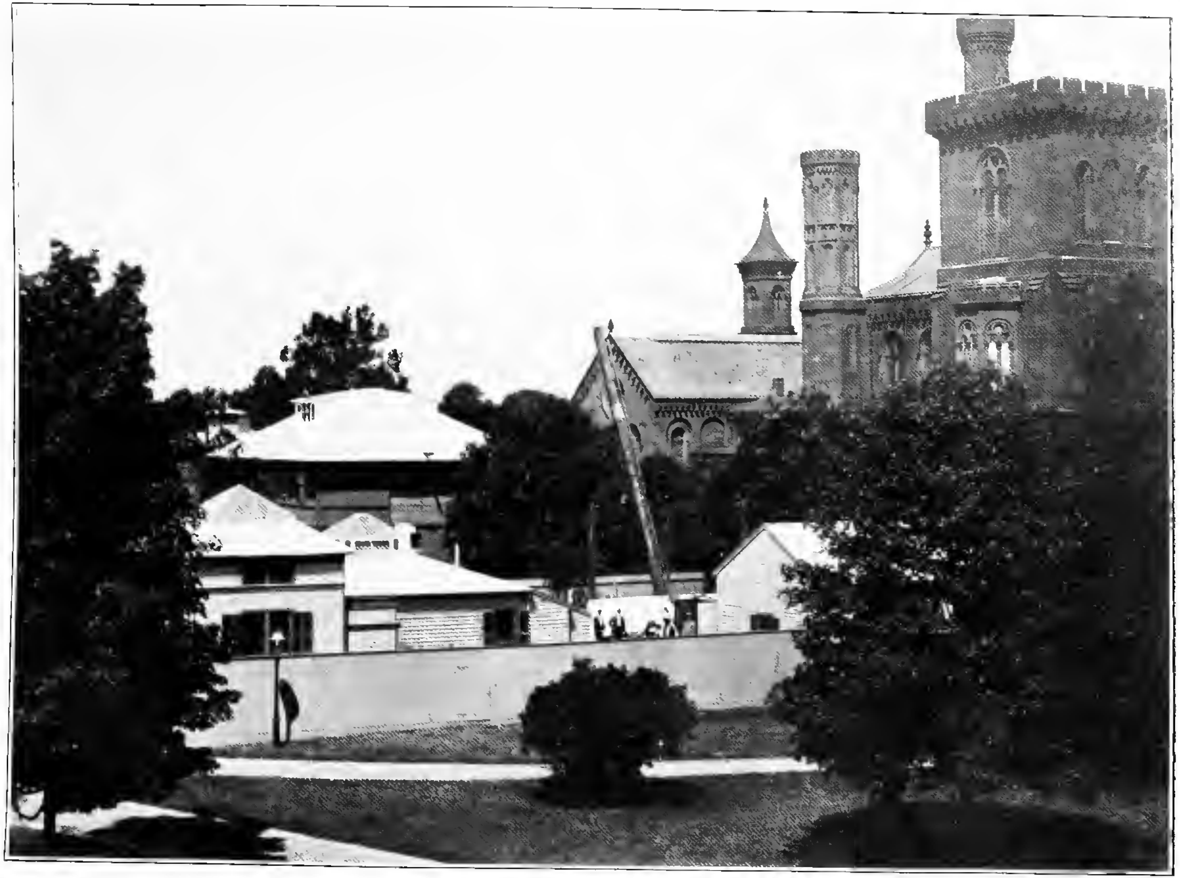
The first chart represents the distribution of heat in the prismatic spectrum as measured by the bolometer at two different times of the afternoon of April 17, 1903. Other spectrum energy curves were taken on that and many other days representing the spectrum under various conditions, and from these curves the rate of decline of heat in the different rays could be studied as the sun sunk lower and lower in the west. Upon the second chart is shown how well the observations of atmospheric transparency made on two different days and at four different points in the spectrum accord with the theory. Exact accord would be shown if the observed points lay precisely upon the straight lines drawn in the figure.

The next chart gives the results of many days observations of the transmission of the atmosphere. Heights on this chart represent the fraction of the solar ray which could pass through the atmosphere if the sun were directly overhead, and horizontal distances represent the wave lengths of the light. It appears from this chart that the air was much more transparent in the years 1901 and 1902 than it was during the first eight months of 1903, but that more recently it has become more clear and has almost reached the transparency it had in 1902. Perhaps so marked a change as this may have influenced temperature and the growth of crops in 1903 very materially, although this subject is not yet thoroughly investigated.

Another chart shows the distribution of solar radiation on the wave length scale outside the atmosphere. From the position of the highest point of the curve at a wave length of 0.49μ it is inferred that the temperature of the sun itself is nearly 6000° Centigrade or over 10000° Fahrenheit. The hottest things on the earth, such as the electric arc light, do not probably exceed 3500° Centigrade. Inasmuch as bodies radiate nearly as the fourth power of their temperatures above absolute zero (—273°C.), it follows that an electric arc light of equal area to the sun would only send out about one-sixth as much heat as the sun does.

By the aid of bolographic studies of the transmission of the air, numerous determinations of the intensity of the sun's heat outside the atmosphere have been completed for the year 1903. These results are shown in the next chart and are connected by a red line drawn from one observation to the next. Below them is shown in black the mean *departures in temperature* during 1903 from the normal temperature representing the mean of many years of observation at 89 stations distributed over all the land areas of the north temperate zone, including the United States and Canada, Insular and Continental Europe, North Africa, and Siberia. It may be said of the two charts that they both exhibit a remarkable falling off about April 1, 1903, and that the fall of temperature would be a natural result of the observed fall in solar radiation. But the subject is one of much difficulty, and the period of observation still far too short to warrant more than the conclusion that further study may prove of the highest importance in foretelling the changes of climate on which the conditions of coming harvests depend.

The immense importance to us of such foreknowledge as will enable us to forecast the harvests and in other ways look into the future need hardly be insisted on if we can get it, and we apparently can get it only from such studies as these, which is a justification of the existence of our astrophysical observatory.



The Smithsonian Astrophysical Observatory.